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LECTURES

ON

ELECTRICITY.

BY

G. C. MORGAN.

VOL. II.

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LECTURES

ON

ELECTRICITY.

VOL. II.

ON THE CONDUCTING POWERS OF DIFFERENT BODIES.

OUR apparatus has hitherto conſiſted of a glaſs cylinder, a common conductor, with the addition of a Leyden phial; or it has conſiſted of an electrical ſurface and a ſcudo. All the effects which have been deſcribed and elucidated, have been merely the conſequences of diverſifying the magnitude of theſe inſtruments; the dimenſions of their ſeveral parts, and their ſituations; we ſhall now advance to a branch of electricity in which the machinery becomes neceſſarily much more complicate, and by which a new and very important relation of the various bodies in nature is to be inveſtigated.

A very brief recurrence to a fact, which has already engaged much of your attention, will uncover a vaſt accumulation of effects. Every addition of the electric fluid, to any charged body, is always attended with a correſpondent and equal loſs, and the reſtoration of the addition, ſo as to make up the loſs, is called reſtoring the equilibrium. This effect is produced by forming a paſſage, thro' which the addition will paſs, or, to uſe the language of electricians, *by forming a circuit.* When the outſide and the inſide of a charged phial are connected by a wire of metal, the quantity collected on the inſide paſſes through this wire as its circuit. When the circuit is formed of different materials, very different effects are obſerved. Some will allow a greater part of the charge to paſs through than others, or they afford an eaſier paſſage than others; or, to uſe the language of electricians, ſome are *better conductors of the fluid than* *others*. I have beſtowed a great deal of pains on my attempts to determine the degrees of this conducting power, and to inveſtigate ſome general cauſe of its diverſities in different bodies. The ſucceſs of my endeavours has not been fully adequate to my wiſhes. I have, however, done ſomething which may aſſiſt others, and perhaps contribute to your improvement. I will deſcribe, firſt of all, the machinery I uſed, and the principles on which it was conſtructed.

FIG. XXVII.

ABECD is my electrometer, or the inſtrument which I employ to meaſure the preciſe charge which I convey through the body to be examined. I would be far from calling it an improvement of that made by Mr. Brooke; it is an alteration only which I found moſt ſuitable to the peculiarities of my own purſuits. CE is a very light braſs tube, with a very light braſs ball annexed to the extremity of it: the dimenſions of the ball are one inch and a quarter in diameter. The tube is eight inches in length, and one inch and a quarter in thickneſs: this tube is annexed to a multiplying wheel, which is connected with the hand on the dial-plate, and by its motion gives four circumvolutions of the hand to every one-fourth of the tube. The figure diſcovers to you, that this tube reſts upon another ſimilar tube, which is fixed. The moveable tube has a ſlider upon it, which, by moving backward and forward, regulates the weight to be raiſed, when the electrometer is connected by a metallic communication to the inſide of a jar, while charging. The inſulation of the tube, balls, and the watch-work annexed to them, is obvious from the figure, in which AB repreſents a ſtem of glaſs.

My reaſons for making this inſtrument different from that invented by Mr. Brooke, were the following: I found that the leaſt difference of charge, when the phial was highly loaded, produced a moſt amazing irregularity in the effect dependent upon the paſſage of the charge. Hence, though the arm was regulated ſo as to ſhew the difference of grain weights, ſtill I experienced a difficulty in aſcertaining *when* exactly the two balls were ſeparated, and I found conſiderable diverſities dependent on the greater or leſs ſeparation of them. I therefore connected the arm with the hand on the plate, and was thus able to determine accurately the very inſtant in which the ſame quantity of charge might be made to paſs through any given ſubſtance. It may not be amiſs here to mention my diſappointment in attempting, by Lane's electrometer, to determine the preciſe charge which paſſed through any body at ſeveral different times; or, in other words, to be able, in two or more different experiments, to ſay that I uſed exactly the ſame charge.

FIG. XXVIII.

AB is a wire, with two balls at its extremities. It moves backward and forward in the ball C, and by this means may be fixed at any given diſtance from the braſs ball D, which is immediately connected with the inſide of the phial or battery, while the connexion with the outſide is continued from the wire AB. All the parts are united into one inſtrument by the bent glaſs ſtem FG.

I have endeavoured to aſcertain the accuracy of this electrometer, by comparing the reſults of experiments without number; but I have not yet been able, in three ſucceſſive trials, to make the *ſame force* ſtrike through the *ſame length* of air. The cauſes of this uncertainty proceed from the inviſible conducting atoms which float in the atmoſphere, from the continual intruſion of hairs into the interval between the ſtriking balls, and from the ſudden changes of temperature in the air, together with a variety of other circumſtances, the minuteſt of which has a conſiderable influence on the conducting power of the circuit.

In determining the conducting powers of different bodies, the certainty of our reſults will depend, not more on our accuracy in meaſuring the preciſe charge employed in each experiment, than on the adaptation of the methods we uſe to the conveyance of the whole of that charge through the body to be examined.

Hence proceeds the neceſſity that our diſcharging inſtrument,

1ſt, Should be conſtructed ſo that no other paſſage may be open for the eſcape of the fluid to the outſide of the bottle or battery, than that of the intended circuit.

2dly, That the conducting wires of the inſtrument ſhould come into contact with the inſide of the charged ſurface, as rapidly as poſſible; for, by approaching it *gradually*, part of the charge is taken off previouſly to the exploſion, and conſequently the force very much diminiſhed.

3dly, That the operator ſhould not be in the atmoſphere of the conductor, at the moment of the diſcharge.

4thly, The diſcharging inſtrument, and the inſide of the charged ſubſtance, ſhould be ſeparated as rapidly as they are connected: we ſhall otherwiſe be unable to meaſure the reſidues, and we ſhall find it neceſſary to accumulate a new charge every time we are miſtaken in our ſtriking diſtances. When the charged ſurface is ſmall, the toil of operation is thus doubled. It is certainly increaſed tenfold when the charged ſurface is of conſiderable dimenſions.

That inſtrument which I ſhall now deſcribe is the contrivance of my brother, ſuggeſted by the laſt of a great number of inſtruments of the ſame kind, which I had previouſly made, without ſatisfying myſelf.

THE DISCHARGING ROD.

FIG. XXIX.

A and B are two braſs wheels, whoſe diameter is four or five inches: they are connected by an axis, which turns eaſily in a collar fixed upon the glaſs ſtem, D M. The wires, D C and E F, are ſcrewed into the peripheries of the wheels, but on ſides directly oppoſite to each other. The lengths of theſe wires are regulated by the diſtances at which the diſcharging rod is placed from the conductor: their direction is perpendicular to the axis of the wheels. But two other wires are to be fixed perpendicularly to the planes of the wheels; they are ſcrewed as nearly as poſſible to the periphery of the wheels, but at oppoſite points, ſo that they may ſtrike objects lying in the ſame line, parallel to the axis, at the diſtance of half a revolution from each other. The lengths of theſe wires are regulated by the diſtances at which they join the metallic, or any other connexion that is formed with the outſide of the phial.

The mode of uſing the diſcharging rod is the following:

When C is brought into contact with the conductor, it receives the fluid, and conveys it through G to the outſide of the charged ſurface. The motion of C is not ſtopped by the contact, but the continuance of it brings E into the ſame contact by which the reſidue of the jar is conveyed thro' K to the outſide. The glaſs ſtem ſhould penetrate deeply into each of the caps, for the whole apparatus will be otherwiſe looſened and put out of order, by the neceſſary rapidity of the motion, and the conquaſſation of parts attending it.

If C, in its circumvolutions, ſtrike againſt an immoveable body in connexion with the conductor, it is frequently ſtopped, and then its ball is injured, or a change unfavourable to the accuracy of the experiment takes place.

FIG. XXX.

To prevent theſe inconveniencies, C ſtrikes the ball A, which is connected with the braſs tube that penetrates into the conductor, with an elaſtic wire bent into the form of a ſpring. The edges and points of this inſtrument are rendered impotent by faſtening a box to the braſs tube, ſo that the ball, A, may move backwards and forwards in the hollow of it, when ſtruck by C. The box ſhould be made of hard wood, and its edge carefully turned and well poliſhed.

FIG. XXXI.

The inſtrument I am going to deſcribe, is deſigned for furniſhing the fluid, in its paſſage from one ſide of a charged ſurface to the other, with two circuits, and, from the preference it ſhews, to determine the ſuperior conducting power of the fluid or ſolid through which it paſſes.

AB is a ſtrip of braſs about an inch and a half wide, and half an inch thick. It reſts upon the caps of two glaſs ſtems, and is faſtened by ſcrewing over it the balls A and B.——F and G are two holes, through which the two wires, FH and GI, ſlide very ſteadily, ſo that the diſtances between their extremities, and thoſe of the wires K and L, may be changed at pleaſure, and aſcertained with accuracy.—BM is a wire, whoſe extremity is ſo elaſtic as eaſily to give way when ſtruck by the arm of the diſcharging rod. The wooden caps, into which the glaſs ſtems are cemented, and upon which they ſtand, are connected by a ſtrip of metal or wood, ſo that the whole may form one inſtrument.

The following is the proceſs of operation, when the preceding inſtrument is employed.

The arms of the diſcharging rod, by their contemporaneous contact with the conductor, and with M, Fig. XXXI. enable the fluid to ſtrike from I or H to K or L, which are wires connected with the outſide of the charged ſurface.

Suppoſe that, before it could paſs through A I, it muſt force its way through a body of water, then the charge will rather ſtrike through an interval of three inches between H and K, than through one inch between I and L; and the difference of length in theſe intervals is a meaſure of the ſuperior reſiſtance of the water to the paſſage of the fluid, above that of the metals.

The method of placing the water, or any other fluid or ſolid, in the circuit, will be deſcribed hereafter.

The next inſtrument I ſhall deſcribe, was ſuggeſted by the circumſtances of the following fact.

FIG. XXXII.

A B is a glaſs tube half an inch in diameter; one end of it is cloſed, having the wire AC paſſing through it; the other end, B, is inverted into a baſon of water, in ſuch a manner as to have its edge juſt below the ſurface of the water. The wire DB is connected with the outſide of the charged phial. The fluid, in its paſſage from C to D, produces an expanſion of the air, a large bubble of which eſcapes, and ſoon after the water riſes, and gives a meaſure of the change that has taken place.

The effect produced in this experiment is the conſequence of that adheſion, by which the electric fluid is united to the air in the tube; for when ſuch an adheſion takes place between any two bodies, the motion, communicated to the one, muſt be neceſſarily participated by the other.

The momentum cauſed by the exploſion of a high charge from a conſiderable battery, accords to the amazing velocity of the electric fluid in its paſſage, and varies in proportion to its quantity.

If a charge of ten feet ſurface be uſed, a weight exceeding that of four pounds may be moved.

If the charged ſurface be doubled, the effect is doubled; if quadrupled, the expanſion is fourfold.

The preceding effect is not confined to air. It is produced by the paſſage of the charge through oil, æther, ſpirits of wine and water; for if either of theſe fluids be ſubſtituted in the circumſtances of the air in the tube, and the charge, in its paſſage through either of them, be made luminous, by bringing the terminations of the two conducting wires near enough to each other, a degree of expanſion takes place, which varies according to certain circumſtances dependent on the conducting power of the expanded ſubſtances.

When the conducting power is perfect, the charge meets with no reſiſtance in its circuit. On the other hand, this reſiſtance increaſes as the conducting power diminiſhes, and proceeds from the adheſion of the electric particles that are moved to the body which conveys the charge; but the expanſion likewiſe depends upon the ſame adheſion; the expanſion muſt conſequently vary in proportion to the conducting power. In other words, the conducting power muſt be greateſt when the expanſion is leaſt, and vice verſa.

The following inſtrument, therefore, which is deſigned to meaſure the one, may be accurately conſidered as a meaſure of the other.

FIG. XXXIII.

AB is a glaſs veſſel, whoſe diameter, IK, is four inches, and whoſe length, DC, is two inches and a quarter. AD and BC are two open necks, about three-fourths of an inch in diameter.

EFGH is a braſs cap, having a bore nearly equal to that of the neck; on its narrower end, a cap of cork, I, reſts. When the charge paſſes from one point to another, in the glaſs veſſel, the cork cap, with whatever weight may be placed on it, is raiſed; but the effect is ſo rapid, that the eye cannot determine whether any motion has, or has not, taken place.

The inſtrument repreſented by Fig. XXXIV, is deſigned to remedy this inconvenience. The equal weights, a and b, are ſuſpended by a ſilken thread, which winds over the wheel of a braſs pulley: the hand, e, is fixed on the axis, and expreſſes, by its change of place, the motion of the pulley. A box, carefully freed from all points and edges, contains the wheel, and the whole of the works which may be neceſſary for its movement. To the back part of the box, the blunted wire, cd, is fixed, which, by the aid of the nut, A, preſſing upon it in a perforated cap, may be adjuſted to any required diſtance.

One of the weights of the pulley, at the time of operation, reſts upon the cork cap, Figure XXXIII. It is conſequently raiſed by the expanſion of the air in the glaſs veſſel, and, agreeably to the well known property of the pulley, continues at the height to which it has been moved.

Various were the ineffectual contrivances to which I had recourſe, before the preceding was ſuggeſted by the ingenuity of a friend, who is juſtly celebrated for his ſkill in mechanics.*

The friction of the cork cap may be objected to as a probable ſource of irregularities. My own experience does not juſtify this objection. The reſults of my experiments have not varied much, when I have taken care that the cap ſhould be always placed in the ſame poſition, and when I have attended to ſome other minute circumſtance, which I ſhall mention hereafter. However, the cork cap cannot be uſed, where the conducting powers of the *different kinds* of air, or the expanſions of groſs fluids, are to be meaſured. In ſuch caſes I have always uſed the inſtrument repreſented in Fig. XXXV.

---

* Mr. Wiſeman, of Diſs, in Norfolk.

A is a veſſel ſimilar to what I have already deſcribed. When the airs are examined, it is made of glaſs; but when our experiments are made on oil, æther, ſpirits of wine, or water, it muſt conſiſt of wood; for nothing elſe remains entire after the exploſion. BC is a bent tube filled with mercury, one of whoſe ſurfaces riſes to the bore of the iron ſtop-cock, which is annexed to the neck of the veſſel A. The other ſurface, C, is about an inch and a half below the open end of the bent tube, and, at the time of operation, one of the weights of the pulley, deſcribed in Fig. XXXIV. reſts upon it. D is a ſecond ſtop-cock, with a cap annexed to it. The two ſtop-cocks have a reference to the introduction of different kinds of air into the glaſs veſſel. After this is emptied, by means of the air-pump, I ſcrew to the ſtop-cock, D, a cap, with a bladder tied to it and charged with the air I intend to examine. On taking off a ſtring which ſerved as a ligature around the neck of the bladder to keep in the air, and on opening the ſtop-cock, D, the veſſel becomes filled. I then ſhut the ſtop-cock, and unſcrew the cap annexed to the bladder, and in its room place another, into whoſe orifice the glaſs tube, ED, is cemented; through its cavity, which is almoſt capillary, a ſmall wire paſſes; and when the ſtop-cock, D, is open, it may be puſhed ſo that its point, e, may come within the ſtriking diſtance of the point f. In this inſtrument, it is obvious that the weights raiſed by the expanſion, are proportional to the elevation of the ſurface C.

I have taken great pains to meaſure, by the apparatus I have juſt now deſcribed, a vaſt number of thoſe effects which depend on the non-conducting powers of bodies;

ſo that the accuracy of our concluſions may reſt on the common language of ſeveral diſtinct teſts.

I ſhall give you the experiments made on different ſubſtances with each inſtrument ſeparately.

I ſhall then give you the obſervations which occurred while the experiments were made; and I ſhall conclude this part of our enquiries with an attempt to account for ſeveral of the deſcribed facts by the aid of certain principles which I have already explained.

THE DIFFERENCES OF CONDUCTING POWER WHICH DEPEND ON THE DIFFERENT TERMINATIONS OF THE DISCHARGING WIRES.

The varieties which I have examined, reſulted from conveying the charge through air.

I. *From one flat ſurface to another.*
II. *From one point to another.*
III. *From a flat ſurface to a point.*
IV. *From balls to points.*

I. From one flat ſurface to another.—Some difficulty is experienced in adjuſting the flat ſurfaces, ſo that they may lie perfectly parallel. One of the ſurfaces in my apparatus is that of a large baſin of quickſilver: over this a ground plate of braſs may be accurately ſuſpended by the aid of a common level. The edges of the braſs plate ſhould be carefully rounded off; nor ſhould they hang over the edges of the quickſilver in the baſin; for, when thus ſituated, they always afford an eaſy circuit to the fluid.

Half an inch of air is nearly the greateſt diſtance through which the higheſt charge can be made to ſtrike in theſe circumſtances.

II. From one point to another. ---Some care and trouble are required to keep the points directly oppoſite to each other; for ſo many motions are neceſſary in working the machine, and in making the diſcharge, that ſuch a ſhaking may be communicated to the points as will materially vary their diſtances.

To prevent the cauſe of this inaccuracy, all the parts of the inſtrument, Fig. XXXI. ſhould be tightly connected with each other; ſo that the effects of any accidental motion may at the ſame time be equally communicated to both points. To approximate as nearly as poſſible to the ſame degree of exactneſs in both points, I generally uſe two needles of the ſame, and the ſmalleſt ſize.

I never could make the higheſt

charge of any jar or number of jars paſs between two points in air over a greater ſtriking diſtance than five inches and a half.

Through the ſame length of air, interrupted by ſeveral wires terminating in points, the ſame charge would not paſs.

FIG. XXXVI.

AB repreſents a Leyden jar; D, H, the pointed termination of the metallic circuit, by which it is diſcharged; ab, cd, ef, are ſmall pointed wires, ſo placed that the diſtances, Da, bc, de, taken together, may be equal to five inches and a half. In this caſe the phial could not be diſcharged till the ſtriking diſtance was diminiſhed to three and a half.

I compared the differences between the reſiſtance of an interval, circumſtanced as in the figure, with that of an interval without the intervening wires, by placing them at the ſame time in the two circuits provided by the inſtrument, Fig. XXXI.

The column, A, repreſents the diſtances in which eight points were placed; B, the diſtances in which the fluid paſſed immediately from one point to another.

A	B
1	$1\frac{1}{4}$
$1\frac{1}{4}$	$1\frac{3}{4}$
$1\frac{1}{2}$	$2\frac{1}{8}$
2	$2\frac{3}{4}$
$2\frac{1}{4}$	3
$3\frac{1}{4}$	$4\frac{3}{4}$

By placing a chain in one interval, and a continued piece of wire in the other, the preference given by the fluid, to the circuit through the wire, is a ſtriking evidence of the great reſiſtance made by every chain, even when its links are connected by as much force as can be applied to them.

Theſe facts lead us to expect a conſiderable variety in the reſult obſerved by any two experimentaliſts, who ſhould attempt to make the ſame trials with different machines; for it is ſcarcely poſſible to form a circuit without ſome interruptions ſimilar to thoſe I have juſt deſcribed.

This cauſe of anomaly ſeems to be ſtudiouſly improved by ſeveral mathematical inſtrument-makers, who form their diſcharging rods of a chain paſſing through a bent tube. This contrivance proceeds from the intention of amuſing the eye, by that ſeries of brilliancies which takes place during the diſcharge along the internal ſurface of the tube: but a vaſt deal of force is thus loſt; for I have known the reſidue, in ſome inſtances, amount to one-fourth of the charge.

It was a poſition maintained by ſeveral electricians, during the contention about points and balls, that conducting ſubſtances, placed in any circuit, acted like ſteppingblocks, and conducted the electric fluid through a ſpace of reſiſting medium, much longer than it could otherwiſe penetrate.

Mr. Wilſon aſſerted, that at each new ſurface there was an additional obſtruction, and aſcribed it to a certain ætherial aura, which is diffuſed over all bodies. I do not think the theory, by which he attempts to explain, is ever likely to meet with that evidence by which the preceding experiments juſtify his aſſertion as to fact. It is clear that his *ſuppoſed* obſtruction takes place where the dimenſions of the conducting ſurface are the leaſt poſſible. We ſhall hereafter ſee, that by increaſing theſe dimenſions, we proportionally increaſe the obſtruction, and that balls in the ſame ſituation muſt, as *obſtruents*, be much more powerful than points.

III. The paſſage of the fluid from a flat ſurface to a point.—In my experiments on this ſubject I always ſuſpended a looſe iron wire over an extenſive ſurface of quickſilver. The wire is kept always perpendicular to the quickſilver by its own gravity. The inſide of the jar is connected with the quickſilver, the outſide with the wire. After every experiment, the ſurface of the mercury is to be cleared by drawing over it an edge of thick paſteboard. Without this precaution, a number of conducting fibrillæ will be dropped from the ſurrounding atmoſphere, which, if they happen to reſt any where within the ſtriking diſtance of the wire, will act as points in conveying the charge.

I have obſerved, likewiſe, that after each diſcharge, there is a black ſpeck on the quickſilver, juſt oppoſite to the wire, and I have ſometimes apprehended that this alteration of the metallic ſurface was followed by a ſmall alteration in the length of the circuit.

The greateſt ſtriking diſtance, through which a full charge would paſs from a flat ſurface to a point, was one inch and three-eighths.

When the choice of two circuits was furniſhed by the inſtrument, Fig. XXXI. the relative diſtances, at which the fluid would prefer neither, were the following:

	FLAT SURFACE. *to a point.*		POINTS. *to points.*
Inches.	1¼	*Inches.*	3¼
	1		2⅜
	¾		1⅝
	½		⅞
	¼		⅝

When the flat ſurface was placed oppoſite to a ball three inches in diameter, the greateſt ſtriking diſtance was three-fourths of an inch.

IV. From balls to points.—A repreſents, in the following table, the diameter of the ball, in parts of inches; B, the ſtriking diſtance from the ball; and C, the ſtriking diſtance, when it preferred the point circumſtanced as in Figure XXXI.

A	B	C
$\frac{7}{8}$	$3\frac{3}{8}$	$3\frac{4}{8}$
$1\frac{1}{2}$	$3\frac{1}{8}$	$3\frac{3}{8}$
$2\frac{1}{2}$	$2\frac{7}{8}$	$3\frac{3}{8}$

When one circuit only was open, the *greateſt ſtriking diſtance*, both of the points and the balls, was much increaſed.

In the following table, A repreſents the diameter of the ball; B, the *greateſt ſtriking diſtance* of the ball; and C, the *greateſt ſtriking diſtance* of the points.

A	B	C
$\frac{7}{8}$	$4\frac{5}{8}$	5
$1\frac{1}{8}$	3	0

The ſame charge was uſed in each of the preceding experiments, and in conſequence of taking in every inſtance, the higheſt charge poſſible, I found that a very ſmall difference in this particular was attended with a very ſenſible difference in the ſtriking diſtances.—The following table, therefore, is deſigned to ſhew the varieties reſulting from the uſe of different charges.

A, in the table, gives the diameter of the ball; B, the diſtance which the fluid paſſes over from the balls as eaſily as from the points, or ſo as to divide the charge with the points, when at the diſtances repreſented by C. In D, oppoſite to the the reſults, is placed the quantum of the charge in grains of repulſion, as meaſured by Brooke's thermometer.

TABLE II.

A	B	C	D	
INCH	INCH	INCH		
$\frac{1}{4}$	1	1	30	
$\frac{1}{4}$	1	$1\frac{3}{4}$	15	
$\frac{1}{4}$	$1\frac{1}{2}$	$1\frac{1}{2}$	30	
$\frac{1}{4}$	$1\frac{1}{2}$	$2\frac{3}{8}$	15	
$\frac{1}{4}$	2	$2\frac{3}{8}$	30	
$\frac{1}{4}$	2	$2\frac{5}{8}$	15*	*greateſt ſtriking diſtance of the half charge.*
$\frac{1}{4}$	$2\frac{1}{2}$	$2\frac{7}{8}$	30	
$\frac{1}{4}$	3	$3\frac{1}{4}$	30	
$\frac{1}{4}$	$3\frac{1}{2}$	4	30	

TABLE II. CONTINUED.

A	B	C	D	
INCH	INCH	INCH		
½	1	1	30	
½	1	2½	15	
½	1½	2	30	
½	2	2⅞	15	*greateſt ſtriking diſtance of the half charge.*
½	2½	2⅞	30	
½	3	3⅛	30	
½	3½	4⅜	30	
INCH	INCH	INCH		
¾	½	⅞	30	
¾	½	2⅜*	15	*greateſt ſtriking diſtance of the half charge.*
¾	1	1¼	30	
¾	1½	2⅝	30	
¾	2	4	30	

TABLE II. CONTINUED.

A	B	C	D	
INCH	INCH	INCH		
1	$\frac{1}{4}$	$\frac{3}{8}$	15	
1	$\frac{1}{2}$	$\frac{5}{8}$	30	
1	$\frac{1}{2}$	1	15	
1	1	1	30	
1	1	3	15	*greateſt ſtriking diſtance of the half charge.*
1	$1\frac{1}{2}$	3	30	
1	2	$4\frac{1}{4}$	30	
INCH	INCH	INCH		
$1\frac{1}{4}$	$\frac{8}{16}$	$\frac{11}{16}$	30	
0	$\frac{1}{2}$	$2\frac{7}{8}$	15	*greateſt ſtriking diſtance of the half charge.*
0	1	$1\frac{3}{16}$	30	
0	$1\frac{1}{2}$	4	30	
INCH	INCH	INCH		
$1\frac{1}{2}$	$\frac{4}{8}$	$\frac{6}{8}$	30	
0	$\frac{1}{2}$	$2\frac{1}{8}$	15	*greateſt ſtriking diſtance of the half charge.*
0	1	$4\frac{1}{8}$	30	

OBSERVATIONS.

1ſt, The preceding experiments prove that the nearer a ball approaches to a point, the greater is the ſtriking diſtance; for, by examining the tables, we find that a ball, whoſe diameter is one-fourth of an inch, will convey a charge through a ſpace of air four times longer than the ſpace thro' which the ſame charge can be conveyed by a ball, whoſe diameter is one and a half, or ſix times greater than the other.

2d, By leſſening the charge in moſt caſes, the preceding difference becomes more perceptible. When the ball was three-fourths of an inch, half a charge would paſs from the point through two inches three-eighths of air, rather than through half an inch from the ball.

3d, The ſeries of differences between the ſtriking diſtances of the points, and the balls with a *full charge*, are the following:

A						
$\frac{1}{4}$	0	0	$\frac{3}{8}$	$\frac{2}{8}$	$\frac{4}{8}$	0
$\frac{1}{2}$	0	$\frac{4}{8}$	$\frac{1}{8}$	0	$\frac{1}{8}$	$\frac{7}{8}$
$\frac{3}{4}$	$\frac{2}{8}$	0	$\frac{9}{8}$	0	$\frac{16}{8}$	0
1	0	0	$\frac{12}{8}$	0	$\frac{18}{8}$	0
$1\frac{1}{4}$	$\frac{3}{16}$	0	$\frac{20}{8}$	0	0	0

The figures in column A, oppoſite to each ſeries, repreſent the diameters of the balls, and the firſt figure in each ſeries gives the difference between the ſtriking diftances of the balls and points, when that of the balls is one inch. You will obſerve, that the increaſe in the differences correſponds with the increaſe of the diameters of the balls, and that, after a certain diſtance is paſſed over, the differences increaſe much more rapidly than thoſe of the leaſt ſtriking diſtances. I call your attention to theſe particulars, as the eaſy explanation of them, furniſhed by the theory which I ſhall now offer to your conſideration, is a preſumptive proof that the theory may be true.

The electrical particles, which are to be moved previouſly to the paſſage *of any charge, muſt take the figure of their ſituation from the ſhape of the conducting body that conveys the charge.*

FIG. XXXVII.

1ſt, If the charge paſſes from one point to another, or from a to b, they will lie in the form of a cylinder, whoſe baſis, is nearly the ſurface of the points, and whoſe length is the diſtance of the points.

FIG. XXXVIII.

2d, If the charge paſſes from a round flat ſurface, ab, to a point, c, then the fluid to be removed will form a cone, whoſe baſis is the flat ſurface, and whoſe length will be meaſured by a perpendicular, drawn from c to the ſurface.

FIG. XXXIX.

3d, If the charge paſſes from a ſurface, ab, that is ſpherical, then the particles to be removed will be a ſolid, abc, whoſe baſis are the dimenſions of ab, and whoſe altitude is a perpendicular let down from c upon a tangent to ab.

FIG. XL.

4th, If the charge paſſes from one round flat ſurface to another, the particles to be removed will be the cylinder, ABCD, which will approach nearer and nearer to a cone, as CD diminiſhes.

If the preceding ſtatement be admitted, to determine the different lengths of the ſpark, we have only to compare the different dimenſions of the preceding figures; for the reſiſtance made to the paſſage of the charge will be exactly meaſured by them.

To make the reſiſtance between the two points equal to what it is between the two flat ſurfaces, you muſt increaſe their diſtance till the number of particles, lying between them, becomes equal to the number exiſting between the two ſurfaces.

To make the reſiſtance between a plane and a point equal to that which meets the charge between two planes, the diſtance muſt be increaſed till the cone of particles that forms the medium in the one caſe becomes equal to the cylinder that forms it in the other.

The reſults of our experiments correſpond very much with the preceding ſpeculations.

When balls are uſed, it is obvious that as their ſphericities increaſe, they approximate to a point, and conſequently, that as they approximate to a point, the length of the ſpark, agreeably to our experiments, muſt neceſſarily increaſe.

I will, however, beg leave to amplify on the circumſtances of the charge in its paſſage from a ſpherical ſurface, as I wiſh to account for ſome peculiarities obſervable in the ſeries of differences between the ſtriking diſtances of the balls and points

FIG. XLI.

Let the point which is to receive the charge be at f, the apex of the figure f e a b c d f, and let the particles within its limits be juſt ſo much as the power of one charge can move; then it is clear that they will be moved with *nearly* as much eaſe as a line of the ſame number of particles. If the point be moved to g, then the ſame power will be neceſſary to move the particles contained in the fig. gcd, as is equal to the removal of a line of the ſame number of particles, and the differences of the lines will be nearly as the dimenſions of the figures to each other. But as all ſuch figures, having the ſame baſes, are in the proportion of their altitudes, we have ſtill to find the reaſon why the difference between the effects of the balls and points become greater and greater, as the diſtances increaſe; and why, at a a certain diſtance, the preference given to the points becomes at once much greater than at any other diſtance.

1ſt, It is obvious that the particles in the line o f, Fig. XLI. are moved with greater eaſe than the particles in the line e f; for the diſtance is ſhorter, and in caſe there was fluid enough in the line o f, then no particles would be removed from e f; but when this cannot happen, then, to anſwer the demands of the charge, the extent of that ſurface muſt be increaſed, from which an equal portion of the fluid is removed, in order to make way for that which is on its paſſage: in other words, the baſe, eo, muſt be enlarged in proportion to the magnitude of the charge.

But in ſuch a caſe, independent of the reſiſtance cauſed by increaſing the number of moveable particles, there is another reſiſtance which takes place, owing to the increaſing lengths of e g, or e f, whoſe extreme particles are diſplaced with greater difficulty than thoſe which are ſituated nearer the point f. It is hence obvious, that the further you remove your point f, the more you magnify the ſecond cauſe of reſiſtance which I have ſpecified, and, conſequently, the larger muſt the differences be between the ſtriking diſtances of the ſame charge.

From the reaſoning I have now employed, we may, I think, eaſily perceive why the differences in the ſtriking diſtances, when low charges are uſed, are ſo rapid in their increaſe.

But, 2dly, When you remove the point beyond the greateſt ſtriking diſtance of a ball, or beyond f, Fig. XLI. the debilitating influence of having two circuits open at once is done away, and the charge paſſes from one *point to another*, juſt as if one paſſage only were open to the fluid: conſequently, beyond f, the difference between the ſtriking diſtance of the points and ball muſt be great in proportion to the decreaſe cauſed by keeping two circuits open at the ſame time.

There is but one ſingularity in the preceding experiments, to which the explanation I have given does not apply. From them it appears, "that pieces of metal, introduced into an interval of air, ſhorten the ſtriking diſtance, though each piece of metal be terminated with the accuteſt point." There is certainly ſome reſiſtance in the metal: but this is not ſufficient to account for the effect, as the charge will paſs through a quantity of metal much greater than that uſed in the experiment, rather than through a ſpace of air equal to the diminution cauſed in the interval of the circuit. In caſe the charge, when paſſing from one point to another, removed no particles but thoſe which are ſituated in a ſtraight line between the points, then I ſhould ſuppoſe that the intervention of the metal could cauſe no difference: but the reality is what I have before ſtated. The particles removed are a cylinder, whoſe baſis is conſiderably larger than the dimenſions of the point. Nothing proves this more clearly, than the track marked out upon glaſs by the charge. It is always a broad ſtrip, and may be conſidered as giving the diameter of the figure made by the fluid in its paſſage. But if there be a divergency at each diſtinct point, there muſt be a correſpondent loſs of force, and, however ſmall that loſs, it is obvious that it will increaſe as the number of points increaſes.

THE CONDUCTING POWER OF WATER.

In our inveſtigation of the conducting powers of water and other fluids, we ſhall attend to the following objects:

1ſt, *The reſiſtance made by columns of them, differing both in length and diameter.*

For this purpoſe, I placed tubes of different dimenſions, filled with the ſeveral fluids, in the two circuits afforded by the inſtrument deſcribed in Fig. XXXI. The bore of the tube determined its diameter, and two wires, inſerted through the extremities, enabled me to regulate the length of the column.

The reſiſtance I eſtimated from a compariſon of the interval of air through which the fluid would paſs, after having gone thro' the water, with that interval of air, which the very ſame charge would paſs thro', when conveyed by metallic conductors. The method of placing the tubes in the circuit, is the following.

FIG. XLII.

ABCD is part of the inſtrument repreſented in Fig. XXXI. In the braſs rod, extending from A to B, two holes are made, large enough for the inſertion and eaſy motion of the tubes, which may be kept ſteady, in any given ſituation, by little wedges of wood, forced into the ſpace between the ſide of the tube and the braſs. BC and AB are two glaſs ſtems fixed into the balls A and B, and ſupporting the wire PO, againſt which the wires preſs, that enter the water. The wire, which the diſcharging-rod meets in its circumvolution, is removed to O; for, by receiving the fluid in this ſituation, it conveys it through the tube, either to the points H or F, where the preference given by one of the two circuits is meaſured. The beſt wires you can uſe in theſe ex-experiments are thoſe of which needles are made. You may fit them to the orifices, ſo as to be perfectly tight, by twiſting around them a fine thread of cotton or ſilk. The orifices muſt be ſtopped up with bees-wax, when the tubes are filled with the acids.

2dly, In the preceding method, when the points which enter the water are at certain diſtances from each other, a *part only of the charge* paſſes through. But when the points of the wires approximate within that diſtance, the whole charge paſſes through with great brilliancy of light, and with a violent diſperſion of the glaſs. But the varieties of diſtance, at which theſe effects take place in different fluids, are determined by their conducting powers; it ſhall, therefore, be our next object *to aſcer-*

tain theſe diſtances in a few out of the many diverſities of circumſtances which might be examined.

3dly, Every body is expanded by the paſſage of the electric fluid. Its degree of expanſion muſt, as I have already obſerved to you, depend upon its conducting power. We ſhall, therefore, attempt to meaſure it, and to ſpecify thoſe varieties which proceed from difference of charges, and from ſome other active circumſtances that occur in theſe experiments.

4thly, The quantity of electric fluid, which is conveyed through any body, muſt be leſſened in a certain proportion to the reſiſtance made by that body. The quantity left in the jar, therefore, will be increaſed or diminiſhed, as the reſiſtance is increaſed or diminiſhed. In other words, to determine the conducting powers of bodies, we ſhould endeavour to meaſure the different reſidues that are left in the jars after the diſcharge, when different bodies are placed in the circuit.

5thly, The power of certain fluids, when ſpread over extenſive ſurfaces, to lengthen the ſtriking diſtance of the charge, is no other than their conducting power. We ſhall conſequently attend to its various effects, and, if poſſible, to aſcertain the circumſtances by which they are diminiſhed or magnified.

I. The experiments, whoſe reſults are given in the following table, were made with tubes whoſe diameter was one-eighth of an inch.

A repreſents, in inches, the length of one of the two columns which are placed in the ſame circuit. B repreſents the length of the other column; C, the ſtriking diſtance in air, when it preferred paſſing through the ſhorteſt column; D, the ſtriking diſtance in air, when it preferred paſſing through the longeſt column. The charge, in every experiment, was the ſame, and the higheſt the glaſs would bear; and I give that limit of the preferences within which the charge divided into two ſtreams.

A	B	C	D
10	1	2	1
5	1	$\frac{8}{10}$	$\frac{7}{8}$

When one of the tubes was taken away, and a ſingle circuit only allowed for the charge, the greateſt ſtriking diſtance, through a column five inches long, was two inches and one-tenth; through a column *ten inches* long, it was one inch and a half.

The preceding reſults are the conſequence of examining one column of water with another. The following are the reſults of comparing the intervals of air which the charge preferred, when one of the circuits was formed by metal terminating in points, and the other by a column of water.

The diameter of the tube one-eighth of an inch. A repreſents the diſtance of the points in water; B, the ſtriking diſtance when it preferred the water; C, the ſtriking diſtance when it preferred the metal.

A	B	C
1	$1\frac{7}{10}$	$2\frac{3}{10}$
6	0	2
6	1	$2\frac{3}{10}$
9	0	$3\frac{1}{10}$

When the diameter of the tube was one-ſixteenth of an inch, the wires in the water were brought ſo as to be three-tenths only of an inch diſtant from each other. In that caſe, the fluid would prefer paſſing through its greateſt diſtance in air, rather than through the water, though no interval of air was left in the ſame circuit with it.

If the diſtance of the points in the water were half an inch, and no interval of air in the ſame circuit, the charge would ſtrike through two inches of air, rather than through the water.

II. In the following table are repreſented the comparative diſtances preferred by the charges when two circuits are opened; in one of which the points within the water are brought within the ſtriking *luminous* diſtance of each other, or that *diſtance at which the charge breaks the glaſs.* In the other, the communication is perfectly metallic, excepting the interval of air which meaſures the reſiſtance.

A repreſents the diſtance of the points in water; B, their diſtance in air. Tube, one-eighth of an inch in diameter.

A	B
$\frac{1}{2}$	$1\frac{7}{10}$
$\frac{1}{4}$	$1\frac{1}{10}$

Tube, one-ſixteenth of an inch in diameter.

A	B
$\frac{3}{10}$	$1\frac{3}{10}$

When the points are removed

to a diſtance in the water, exceeding three-tenths of an inch, the glaſs cannot be broken by any charge.

III. The conducting power of water, when meaſured by the expanſive force of the charge, may be examined by two methods.

1ſt, The charge may be conveyed through water, and the expanſion cauſed in an interval of air, placed in the ſame circuit, may be meaſured.

I have made this experiment in circumſtances the moſt advantageous to the water, and I found that, after a charge had paſſed through a veſſel containing ſeveral gallons of water, the expanſion it cauſed in the interval of air was about one-ſixth of what took place when the water was not in the circuit.

2dly, The charge may be conveyed through the water, and the expanſion cauſed in the water itſelf may be examined, by means of the inſtrument deſcribed in Fig. XXXV. When the points were about half an inch diſtant from each other, and the cavity containing the water was a cylinder, whoſe altitude and diameter were each one inch, the expanſion produced by the exploſion did not exceed a fortieth part of what the ſame charge produced in the ſame dimenſions of air.

IV. The following table gives the reſults of thoſe experiments in which I have meaſured the conducting power of water by the reſidues of charges.

A gives the diameter of the tube containing the water; B, the diſtance of the points in the water; C, the interval of air in the ſame circuit with the water; D, the interval of air through which the reſidue would ſtrike. All the figures referred, inches or their parts.

A	B	C	D
$\frac{1}{2}$	9	$3\frac{1}{4}$	$\frac{1}{15}$
$\frac{1}{4}$	9	$3\frac{1}{4}$	$\frac{5}{10}$
$\frac{1}{8}$	9	1	$\frac{1}{2}$
$\frac{1}{16}$	9	$1\frac{1}{2}$	$\frac{7}{10}$

There are ſome anomalies in the preceding experiments, which I could not avoid. An examination of them, however, muſt diſcover to you, that the ſtriking diſtances of the firſt diſcharges may be equal, and yet their reſidues different; or that the ſame *quantity* of electricity is not neceſſary in all caſes to produce the ſame effect.

V. The relation of their conducting power to the ſurface, or a thin lamen of water, is next to be examined.

1ſt, If a ſtrip of water be drawn over a ſurface of glaſs, with due care that all parts of it be contiguous to each other, the ſame charge will paſs over three times the length on this ſtrip, which it would ſtrike through from one point to another in air.

2dly, If this ſtrip were widened, ſo as to cover a ſurface ten inches ſquare, the length of the ſtriking diſtance would not be altered.

3dly, When the water is more than half an inch deep, it does not ſtrike to ſuch a diſtance as when it is thinly ſpread; I found the difference to be nearly that of eleven to nine.

In the preceding experiments, the metallic points were always placed on the ſurface of the water, and that only was conſidered as a perfect diſcharge, whoſe noiſe and brilliancy were exactly ſimilar to thoſe of the fluid's paſſage through air.

I conclude my experiments on the conducting powers of water, by examining whether the different termination of metallic bodies that are immerſed in it, produce varieties in the ſtriking diſtance, ſimilar to thoſe obſervable in air.

Dr. Prieſtley's attention was ſeveral years ago attracted by that ſtar which appears on the ſurface, when the point that conveys the fluid is ſuſpended juſt over the water. This appearance will be found, by cloſe examination, to conſiſt of ſeveral radii, diverging from the part over which the point hangs, and reſults evidently from the effort of the charge, in conſequence of finding too much reſiſtance in the ſpace juſt below it, to ſecure other paſſages of leſs reſiſtance. I therefore concluded, that if the metal touched every part of the ſurface where the ſtar was viſible, the paſſage of the charge would be neceſſarily facilitated, and that in water the electric fluid would ſtrike rather from a plane than a point. This is exactly the reverſe of what takes place in air.

Expt. Two cylindrical glaſs veſſels, whoſe diameter equalled four, and altitude three inches, were filled with water. The bottom of one of the veſſels was a plate of braſs; that of the other was made of a non-conducting ſubſtance, having a pointed wire paſſing through the centre. I took care that the diſtances between the conducting terminations in both ſhould be exactly equal, having previouſly placed on the ſurface of the water in the veſſel a plate and a metallic point. Both the veſſels were ſituated according to the directions given for the uſe of the inſtrument deſcribed in Fig. XXXI. By repeated trials I found that the ſame charge would always prefer paſſing thro' the water which was bounded by two planes. The difference of reſiſtance, however, in the different veſſels could not be very conſiderable; for I found that the leaſt alteration in the length of the air interval, through which the charge paſſed after it had ſtruck through the water, gave the preference to *either of the circuits*.

In the preceding experiments I gave a certain degree of attention to the temperature of the water; for I diſcovered that varieties in this circumſtance were productive of varieties in the conducting power. I generally took care that water ſhould raiſe Fahrenheit's thermometer to 60°. The relation of the temperatures to the conducting powers of fluids opens a wide field of enquiry, and thoſe who wiſh to purſue it will feel the neceſſity of much toil and invention in adapting their apparatus to their purpoſes.

In the progreſs of ſome experiments which I made on boiling water, I found the conducting power to be ſo much increaſed, as ſcarcely to admit the poſſibility of making any charge luminous in its paſſage through the heated medium, though the points of the inſerted wires were brought almoſt into contact with each other.

The pulverized particles of nonconducting ſubſtances, when ſuſpended in water, produce no change in the conducting power. If the water be confined in a ſmall ſpace, they operate by leſſening the dimenſions through which the charge can paſs. My experiments were chiefly made on wet ſand. When a quantity, moiſtened into the conſiſtence of a thick ſyrup, was put into a tube about half an inch in diameter, the charge which paſſed thro' was very faint, and not equal to one-tenth of what paſſed through a metallic communication.

The quantity of acid or ſaccharine matter, contained in the juice of trees or plants, is in general ſo ſmall, that their conducting power is not at all ſuperior to that of the water they contain. Hence proceeds the impoſſibility of conveying any charge through twigs and branches, without lacerating their ſeveral parts, or without bringing the metallic points within a diſtance of each other, much inferior to that through which the ſame charge would ſtrike in air. For in plants, and moſt other vegetables, the juices circulate in canals that are capillary, and water is ſo far from admitting a charge to paſs through it in a capillary tube, that it will ſcarcely ſerve as a circuit when the diameter of the tube is one-ſixteenth of an inch. It ſhould be obſerved, that the moiſt, ſmall ſurface of twigs and branches, which lies juſt under the bark, lengthens the ſtriking diſtance very conſiderably.

From the reſults and circumſtances of the experiments, which relate to the conducting power of water, we may, I think, be furniſhed,

1ſt, With the explanation of a fact, whoſe ſingularities have engaged the attention of ſeveral electricians, and more particularly of Mr. Henley, who has detailed the circumſtances of it very minutely in the Philoſophical Tranſactions.

When the maſts of ſhips have been damaged by lightning, the fluid has always paſſed over that part which was painted or covered with a coating of lamp-black and oil. This peculiarity is analogous to the preference given by the electric fluid to a paſſage over a wet ſurface, rather than through a body of wet wood. The maſts of ſhips are made of deal, which, notwithſtanding its reſinous impregnation, will abſorb thirſtily whatever moiſture falls upon it. The ſame moiſture is retained externally by oily ſurfaces. The lightning, therefore, when ſtriking near ſuch a ſurface, muſt find leſs reſiſtance in its paſſage over it, than in forcing its way through the capillary canals of the wet wood.

2dly, We may, by the preceding experiments, account for a ſingular appearance which attends the ſplitting of trees by lightning. In caſes of this kind, the bark is ſtripped off, even to the extremities of the minuteſt branches; for the electric fluid, directed by the conducting power of the wet ſurface that lies under the bark, paſſes over, and, by its expanſive force, neceſſarily produces the deſcribed effect.

3dly, In ſome of the few inſtances of the devaſtations attending a thunder-ſtorm, which I have been able to examine, thoſe who were immediate ſpectators of the events, have aſſured me, that at the inſtant of the ſtroke, the whole ſurface of the ground ſeemed to be in a blaze. I ſatisfied myſelf with conſidering this appearance as the reflexion of the flaſh, whoſe brilliancy would be naturally brightened by terrified imaginations. In one of the inſtances I allude to, however, the ſpectator deſcribed the ground as ſwimming in the rain which had previouſly fallen. I have, therefore, ſuſpected that the lightning took its courſe over the wet ſurface, and in that caſe the brilliancy would exactly reſemble that which is diſplayed when the charge is conducted over wet glaſs.

I could have lengthened my detail of experiments on the *conducting power of water*; but I apprehend that I have already treſpaſſed on the allowance due to a public lecturer. I muſt likewiſe acknowledge that many of the experiments, which I have repeated before you, are attended with very puzzling anomalies, ariſing from ſome undiſcovered imperfection in the apparatus with which they were made. But as I have led you into the track, I hope you will moſt ſucceſsfully urge the purſuit. The deſiderata, which you ſhould have in view, are the following:

1ſt, To meaſure the reſidues of jars by their expanſive force, I have done it only by their ſtriking diſtances.

2d, To meaſure, in a great variety of inſtances, the diverſity of effect which depend on altering the dimenſions of the tubes, I have done it only in a few, and I have not applied the expanſive force to theſe caſes. It ſtrikes me, that a diligent inveſtigation of the object I am now recommending to you may lead us to an acquaintance with ſome general law of the reſiſtance made by water to the electric fluid.

3d, I have generally uſed the ſame charge in my experiments. Much, I think, may be learned from varying the charges.

4th, My experiments on wet ſurfaces are very particular; they might be much improved, I think, by attending more minutely to quantities, and by placing different quantities in the circuit, ſo that the reſiſtance cauſed by this difference may be meaſured accurately.

SPIRITS OF WINE.

In determining the conducting power of this fluid, the method I obſerved was exactly the ſame with that which I followed in my experiments on water.

I. I compared the ſtriking diſtances of the charge, when conveyed through columns of different dimenſions, or through tubes filled with ſpirits, and varying in length and diameter.

A repreſents the diſtance of the points in the tube; B, their diſtance in the interval of air through which the charge ſtrikes. Tube, one-ſixteenth of an inch.

A	B
1	1½
2	2
3	1½
4	1¼

In the firſt of the preceding experiments, the colour of the charge in its paſſage was yellowiſh. Its courſe, likewiſe, was undivided, and without any ramifications. In the firſt experiment alone, I ſhould therefore conſider the charge as paſſing by one effort; for in the other, the colour of the flaſh was always purple. The courſe had the appearance of a ſtream, and when the points were diſtant four inches from each other, it was viſible only at its extremities. Tube, one-eighth.

A	B
1	2½
2	1¼
3	1½

Tube, one-fourth.

A	B
1	3½
2	2½
3	1¾

I compared the conducting power of ſpirits of wine with that of water, by opening two circuits, in one of which I placed a tube filled with water; in the other, a tube of the ſame dimenſions, filled with ſpirits. The diſtance of the conducting points in both was always equal to one inch.

A repreſents the ſtriking diſtance through the ſpirits; B, the ſtriking diſtance through the water. Tube, one-ſixteenth.

A	B
1¼	2
Tube, one-eighth.	
1	2
Tube, one-fourth.	
1¼	2

In moſt of the preceding caſes, when the diſtances were exactly what they are repreſented to be in the table, the charge divided into two ſtreams; that which paſſed through the ſpirits of wine was ſcarcely viſible, while the other was of a bright, yellow colour, and attended with a very faint ſound.

II. When I conveyed a charge through ſpirits of wine, ſpread on the ſurface of glaſs, the ſtriking luminous diſtance never exceeded five inches in length.

When two circuits were opened on the ſame ſurface of glaſs, one of which was formed by a ſtrip of *water* one-fourth of an inch acroſs the other, by a ſtrip of ſpirits of wine of the ſame dimenſions, the charge would ſtrike moſt brightly luminous over eight inches on the water, rather than over four inches and a half on the ſpirits of wine.

III. A repreſents the breaking

diſtance of the charge in tubes of ſpirits of wine; B, the ſtriking diſtance in the interval of air, when the reſt of the circuit is metallic. The two circuits are open at the ſame time, and there is no interval of air in that formed by the ſpirits. Tube, one-ſixteenth.

A	B
$\frac{3}{10}$	2
Tube, one-eighth.	
$\frac{1}{4}$	2

When one circuit only was open, the greateſt breaking diſtance in the tube, one-eighth of an inch in diameter, was five-tenths.

IV. I meaſured the reſidue left in the jar, when the charge had been paſſed through a tube of one-ſixteenth of an inch in diameter, filled with ſpirits. The conducting points were one inch diſtant from each other, and the ſtriking diſtance of the reſidue was one inch and a quarter. The reſidue of the very ſame charge, when it had paſſed through water of the ſame dimenſions, ſtruck through one-third of an inch of air. The expanſion cauſed by the former reſidue was near three times greater than that cauſed by the latter.

V. I filled a tube with ſpirits of wine, whoſe diameter was half an inch. The conducting points which I placed in it were ten inches diſtant from each other. The charge which paſſed through this column did not produce one-fiftieth the expanſion which the ſame charge cauſed in a metallic circuit.

When the tube was filled with water, the expanſion was rather greater, but the difference, taken from an average of many reſults, was too ſmall to be eſtimated with any accuracy. When I attempted to meaſure the expanſion cauſed in the fluids themſelves by the charge, I experienced a ſimilar approach to equality.

OBSERVATIONS.

1ſt, The *breaking* or *luminous* diſtance is always leſſened by enlarging the diameter of the tube.

This is partly the conſequence of increaſing the diameter of the medium in the one, while you increaſe its length in the other caſe. It is partly, moreover, the conſequence of adding to that conducting power, which, in the larger tube, would have conveyed a part of the charge inviſibly, and almoſt inſenſibly. But as this part is increaſed, that quantity of the electric fluid is diminiſhed, by whoſe reſiſtance and attendant expanſion the glaſs is broken. We ſhall ſee hereafter, that a few particles of acid, introduced into the ſpirits, would produce the ſame effect as that of increaſing the diameter of the tube.

2d, The reſults vary with the

ſpecific gravity of the ſpirits; that which approaches neareſt to pure alcohol, is the worſt conductor. I commonly employed the ſpirits as they are ſold by the druggiſts, and I found little or no variety in their ſpecific weight.

VITRIOLIC ÆTHER.

What I uſed floated on water, and was carefully cleared by an alkali of all the acid which it might otherwiſe have contained.

I. No charge could be made to paſs through columns one-ſixteenth and one-eighth of an inch in diameter, till the conducting points approached within the breaking or luminous diſtance.

II. A repreſents the breaking diſtance in æther; B, the correſpondent ſtriking diſtance in air, when two circuits were opened. Tube, one-ſixteenth.

A	B
$\frac{1}{2}$	$\frac{9}{10}$

Tube, one-eighth.

$\frac{1}{2}$	$1\frac{2}{10}$

The greateſt breaking diſtance in a tube, one eighth of an inch in diameter, was one two-tenths of an inch.

III. When the charge was made to paſs through a certain breaking diſtance, the greateſt expanſion produced within that diſtance was about one-fourth of what the ſame charge would produce in air.

OBSERVATIONS.

1ſt, In experiments on the expanſions, both of ſpirits of wine and æther, a quantity of vapour is formed, and a change of temperature produced. I have therefore found it neceſſary to make every new experiment on freſh ingredients.

2dly, In the experiments made on water, ſpirits of wine, and æther, the expanſion was by no means regulated by the diſtance of the conducting points within the veſſel. The expanſion began at the greateſt luminous diſtance: it increaſed gradually as this diſtance diminiſhed, till it came to a maximum; it then decreaſed, till the points touched. In other words, when the points were three-tenths or one-tenth, the charge was *luminous* in its paſſage; but at both theſe diſtances the expanſion was leſs than when the points were ſeparated two-tenths of an inch.

This diverſity, I aſcribe at the greateſt diſtance to that diminution of the charge which is cauſed by what is taken off ſilently, or what would be taken off, in caſe the charge were not luminous. At the leaſt diſtance, I conſider the diminution as the effect of bringing the wires too near each other. Their particular mode of operation in this caſe, I ſhall explain hereafter.

OILS.

No charge will paſs through oils of any kind, till the conducting points are brought within what I call the *luminous* or *breaking* diſtance of each other. It ſhould, therefore, be our chief object to aſcertain the relation which ſubſiſts between the breaking diſtances and the nature and dimenſions of the oils. The moſt puzzling anomalies for a long time infeſted my experiments on this ſubject. At length I found that every variety of reſult might be avoided by attending to the following circumſtances:

1ſt, If both the conducting points be not immerſed in the oil, a vapour is formed by the action of the charge co-operating with the air that lies contiguous to the ſurface. This vapour frequently takes fire, and produces a change of temperature, both in the veſſel and its contents, by which their conducting power is materially increaſed.

2d, The length of the breaking diſtance is often increaſed by the adherence of ſmall particles of air or vapour to the points of the conducting wires.

3d, Every diſtinct charge is attended, not only with a change of temperature in the oil, but with a ſeparation of coaly matter, from one minute portion of which to another the electric fluid paſſes with much greater eaſe, than through oil when perfectly pure.

4th, If the leaſt moiſture adheres to the ſurface of the veſſel, it will ſoon mix with the oil, and will increaſe the breaking diſtance, moſt commonly to double, and frequently to three times its proper length.

To prevent the irregularities which ariſe from the preceding cauſes, we ſhould always uſe glaſs tubes. For a long time I made my experiments with wood, which I preferred, becauſe it would not break with each diſtinct charge, and becauſe I could be very exact in regulating the bore. But when the ſides of the veſſel are pellucid, we may ſoon perceive the leaſt impurity that may have been mixed with the oil. Moreover, the ſame oil ſhould never be uſed twice; for even one charge produces the coaly matter. Beſides, the oil ſhould be well boiled, and care taken that its temperature is invariably the ſame in each diſtinct experiment.

A repreſents the breaking diſtance in oil; B, the correſpondent ſtriking diſtance in air, when two circuits are open.

OIL OF OLIVES.

Tube, one-ſixteenth.

A	B
$\frac{2}{10}$	$1\frac{2}{10}$

Tube, one-eighth.

A	B
$\frac{2}{10}$	$1\frac{3}{10}$
$\frac{3}{10}$	$2\frac{3}{10}$

OIL OF SASSAFRAS.

Tube, one-eighth.

A	B
$\frac{2}{10}$	3

In this oil, the charge would not paſs when the tube was one-ſixteenth.

OIL OF ROSEMARY.

In this oil, when the tube was one-eighth, the ſtriking diſtance was greater than in any other oil.

The irregularities which attended the expanſion of oil by the paſſage of the charge, were ſo conſiderable, as to forbid any deciſive language on the ſubject.

In my experiments, I have always found it neceſſary to confine the oil by wood; for nothing elſe will reſiſt the violence of the exploſion. When the diameter of the hollow was half an inch, and its length about ſix-tenths of an inch, I obſerved that the expanſion in linſeed oil was about half what it would have been in common air ſimilarly circumſtanced: but when I examined the inſide of the wood, I always found that the charge had paſſed over its internal ſurface, and conſequently had not permeated the ſubſtance of the oil; nor could I diſcover any other method of avoiding this inconvenience, than that of enlarging and lengthening the hollow of the veſſel: but in this caſe the body of the oil became ſo great, that the expanſion was almoſt loſt in it. Sometimes, however, the force will be ſuch as to exceed ten times the greateſt expanſive force of air. This rare appearance is the effect of the oils taking fire, when it has been previouſly heated by ſucceſſive exploſions, and when one of the conducting points, as I have already informed you, is raiſed above its ſurface.

OBSERVATIONS ON THE PRECEDING FACTS.

1ſt, Tho' oils, in their common ſtate, or after expoſure for ſome time to the atmoſphere, will not allow any quantity of the electric fluid to paſs through them, otherwiſe than *luminouſly*: they are, notwithſtanding, very imperfect inſulators. I have frequently immerſed coated jars of glaſs in a body of linſeed oil, and, provided the cylinder was very large, the torrent of electric fluid would paſs into the jars ſo much faſter than the oil could convey it from the inſide to the outſide of the glaſs, that the jars might be charged till they were broken; but after the revolutions of the machine ceaſed, the charge was inſtantaneouſly diſperſed. Mr. Brook thought that he could render the inſulating power of baked wood *permanent*, by boilit in oil. He continued the boiling a whole day, and, during a few hours after this operation, the wood could be excited, and the inſulation was perfect; but ſo rapid was the abſorption of moiſture from the air, that in leſs than two days the oiled wood was altogether deprived of its power.

2d, The degree of reſiſtance in oils to the paſſage of the electric fluid ſeem to have ſome connexion with their ſpecific gravities. Oil of roſemary, in which the ſtriking diſtance is the greateſt, is one of the lighteſt I uſed. Oil of ſaſſafras is the heavieſt, and its ſtriking diſtance is the leaſt.

3d, The *breaking* or *luminous* diſtance in ſpirits of wine and æther increaſed as the diameter of the containing tube diminiſhed. The reverſe took place in oil. In the one caſe, a ſmall portion will paſs through inviſibly, and in proportion to the quantity that is thus taken off, ſo is the diminution of that height of charge which is neceſſary to break the glaſs; but the diminution I allude to increaſes with the diameter of the tube, and hence ariſes the difference obſerved in the experiments.

4th, The power of oils to reſiſt the paſſage of the charge varies with their temperatures. The breaking diſtance lengthens as the heat is increaſed. But it is to be obſerved, that a *battery of one hundred jars will not permeate a greater body of oil than a ſingle jar charged equally high.*

AIRS.

In the experiments I have made on air, the deſiderata I had in view were the following:

1ſt, The relation ſubſiſting between the different lengths of the ſtriking diſtances in air, and the various heights of a charged ſurface.

2d, The diverſities of this relation in the ſeveral kinds of air, and the connexion of the ſpecific gravities and the phlogiſtication of the different airs with their conducting power.

3d, The varieties of expanſive power in the ſeveral kinds of air, when cauſed by different heights of change, and by different lengths of the ſtriking diſtances.

I had minuted down the reſults of ſeveral hundreds of experiments, which I made with a view to theſe deſiderata; but when I examined them by the teſt of frequent repetition, I met with a few only that were not rendered uſeleſs by anomalies. Long after the experiments were made, I diſcovered that the effect of temperature on the paſſage of the charge is much greater than I had ever ſuppoſed; and as I did not attend to ſo conſiderable a ſource of varieties, I am willing to aſcribe to its influence the great irregularities which I have experienced. The following are the only particulars which I can preſent to you with any confidence in their accuracy.

1ſt, The ſtriking diſtance in common air is *more* than doubled or trebled by doubling or trebling the height of the charge. In other words, the fluid, in paſſing from one point to another, or from one ball of given dimenſions to another of the ſame dimenſions, in caſe it ſtrikes over one inch and a half with fifteen grains repulſion, will ſtrike over more than three inches with thirty grains repulſion.

2d, The ſtriking diſtance of any given charge is greater in inflammable than any other air. It is leaſt in the dephlogiſticated marine gas. The former is the lighteſt, and the latter is amongſt the heavieſt of all the gaſes. But it is clear that the ſpecific gravity is not the ſole cauſe of this variety; for the difference of reſiſtance was by no means commenſurate to the difference of weights. The ſpecific gravities of inflammable and common air, and dephlogiſticated marine gas, were as 1,25. 1. 1,007. but the ſtriking diſtances of the higheſt charge, in paſſing through them, were as 4. 3. 1,25.

The reſiſtance of nitrous air approached neareſt to that of the dephlogiſticated marine gas.

All the dephlogiſticated airs reſiſt more than common air, and are at the ſame time heavier. But in this inſtance again, the degrees of reſiſtance ſeemed to have little or no connexion with the diverſities of weights; for when the ſpecific gravity of dephlogiſticated air was to that of common air as 14. to 13. nearly, the ſtriking diſtances were as 4. to 2,25.

3d, With a ſingle jar, meaſuring two wine quarts, and charged to thirty grains repulſion, I expanded common air ſo that it raiſed two pounds weight. In this experiment, the air which was operated upon did not exceed one ounce meaſure.

I obſerved that two jars would raiſe twice the weights, and three jars, three times the weights raiſed by one jar.

The ſtriking diſtances within the expanded air have an effect upon the preceding reſults. The ſame charge, ſtriking over three inches, produces three times the expanſion of the ſame charge paſſing over one inch.

I have not determined the varieties dependent upon increaſing the capacity of the expanded air.

In different airs the expanſion is very different. It is greater in dephlogiſticate air than common air. In inflammable air it is leaſt. It is remarkable, however, that two ſucceſſive exploſions were ſcarcely ever attended with the ſame effect; this I aſcribe to ſome change in the conducting power produced by the firſt exploſion.

I am inclined, by ſeveral circumſtances that occurred in the preceding experiments, to infer that the inſulating properties of the different gaſes are not at all affected by the ſuſpenſion of any additional moiſture. The dephlogiſticated marine gas is compoſed of acid particles, and yet its conducting power is leſs than that of any other gas.

In ſome inſtances, when our experiments were made on nitrous air, a portion of common air was accidentally mixed with it, and the veſſel was filled with red fumes, which are the particles of the nitrous acid ſeparated from its former baſis: but this circumſtance did not lengthen the ſtriking diſtance of the charge.

We may therefore conclude, when a moiſt atmoſphere diminiſhes the power of an electrical apparatus, that it operates by depoſiting a part of its aqueous contents on the ſurfaces of our ſeveral inſulators, and not by any additional conducting power in itſelf.

To obtain a more complete knowledge of the preceding ſubject, or of the relation ſubſiſting between airs and their powers to conduct the electric fluid; concerning which, I have been able to give but a ſmall portion of *ſpecific* information: I think the following objects are thoſe to which your attention may be directed moſt ſucceſsfully.

1ſt, A full aſcertainment of the connexion between the temperatures and the conducting powers of air.

2d, The law, according to which the expanſive force is leſſened when the capacity of the air is increaſed.

3d, We ſhould examine whether all the airs are poſſeſſed of the ſame expanſive force, and whether their ſtriking diſtances are the ſame when their ſpecific gravities are made equal by a proper application of the condenſing machine.

4th, Would not an acquaintance with the different forms of termination, from which the electric fluid ſtrikes when the diſtances are equal in different kinds of air, very much help us in correcting our knowledge of this ſubject?

For inſtance, if we knew the diameter of that ball from which a given charge ſtrikes in inflammable air as eaſily as from one point to another in common air, when two circuits are open, ſhould we not hence derive aſſiſtance to eſtimate the difference of their conducting powers, by applying thoſe principles, agreeably to which, in a preceding part of theſe Lectures, we have explained the effect of points, balls, and flat ſurfaces on the ſtriking diſtances.

ACIDS.

The mineral acids, in the degree of their conducting power, approach neareſt to the metals.

They are the only fluid, excepting mercury, which conveys the charge through a capillary tube: but there is no caſe in which they do not leave ſome reſidue. When the charge is conveyed through columns of them, meaſuring ſome inches in diameter, it is evident, from the light and the ſound of the exploſion, that their conducting power is ſuch as very much to diminiſh either the quantity or the momentum of the electric fluid in its paſſage.

I have confined my examination to the vitriolic acid, from a conviction, juſtified by ſeveral circumſtances, that it differs very inconſiderably from the nitrous and marine acids.

The chief difficulty I experienced was that of conveying the acids into the tubes. I effected this when the tubes were capillary, by placing one end in a body of the acid, and by ſucking at the other. When the acid had riſen to its proper height, I ſtopped up the lower orifice, and then cloſed both orifices with needles dipped in melting bees-wax. If the tube be more than one-eighth of an inch in diameter, the lower orifice may be firſt ſtopped up with a pointed wire, and the tube may be made tight, by melting a little wax on that part of it which is within the tube. The acid may then be poured in without any difficulty.

I. When the tube was capillary, and the conducting points diſtant from each other ten inches, a ſmall portion only of the charge could be forced through the acid, ſo as to ſtrike over ſeven-tenths of an inch in air.

The preceding experiment was made with a jar containing two quarts. When the charge of two ſuch jars was tried, no difference of effect was perceptible.

When two circuits were opened,

the charge would rather ſtrike thro' its greateſt diſtance in air, rather than through the acid, though all the conducting wires in its circuit were in perfect contact.

No charge could be made to paſs, when the length of the capillary was twenty inches; and when diminiſhed to fifteen inches, the paſſage was that of a very ſmall portion attended with every appearance of difficulty.

When the tube was one-eighth of an inch, and the points in the acid twelve inches diſtant from each other, if but one circuit was open, the ſtriking diſtance in air was two inches and a half.

When two circuits were open, the ſtriking diſtance in air

$$\text{Through}\begin{cases}\text{the acid was one inch.}\\ \text{the acid, half an inch.}\end{cases}$$

$$\text{Through}\begin{cases}\text{metal, two inches.}\\ \text{metal, one and a half.}\end{cases}$$

When the tube was one-fourth of an inch in diameter, at all the diſtances of the conducting points within the acid, the charge paſſed over as great an interval of air as through a metallic circuit; but the brilliancy, ſound, and direction were of a ſpark evidently enfeebled.

When the tube was half an inch in diameter, the appearances of the exploſion were ſcarcely, if at all, different from thoſe of the preceding experiment.

II. The greateſt *breaking* or *luminous* diſtance in a capillary tube, filled with vitriolic acid, is equal to two inches.

When the tube is one-eighth of an inch in diameter, the length of the *breaking* diſtance was one-eighth of an inch.

The glaſs could not be broken, nor the ſpark made luminous, in any tube of a larger diameter.

III. When the tube is capillary, the charge, in paſſing through air, produces no ſenſible expanſion.

When the diameter of the tube was one-fourth or half an inch, the expanſion produced was nearly oneninth of what attended the ſame charge in its paſſage thro' metal.

IV. If a ſtrip of the vitriolic acid, one-eighth of an inch acroſs, be extended over a ſurface of glaſs, it will convey the charge brilliantly over a length of ſeventeen inches.

If two ſimilar ſtrips, one of acid, and the other of water, form two circuits, the charge will paſs over twelve inches and a half of the acid brilliantly, rather than over ſix inches of the water.

The dilution of the acid by water increaſes the conducting power, in caſe the proportion of the two ingredients is not greater than that of equality; but when the quantity of water is moſt abundant, then the conducting power is diminiſhed, but never to ſuch a degree as to become equal to that of the water alone.

What I have juſt ſaid of the effect of dilution, is true only when the acid is confined in a tube, or when it is permeated by the charge; for when the acid is ſpread over a non-conducting ſubſtance, and the charge of courſe paſſes over the ſurface, the leaſt dilution leſſens the ſtriking diſtance. I opened two circuits, in one of which was a ſtrip of pure acid, in the other, an equal ſtrip of acid diluted with one-third its weight of water: in this caſe the charge would rather paſs over nine inches of the concentrated, than over ſeven of the diluted acid. This difference, however, was ſenſible only when two circuits were open; for in *one* circuit the ſtriking diſtance of both, with preciſely the ſame charge, was the ſame. A ſenſible difference was perceptible in the ſound and light of the exploſion.

It is to be obſerved, that if the quantity either of concentrated or diluted acid were increaſed in theſe experiments, the electric fluid would then permeate the ſubſtance of the ſtrip, and the diſcharge would not be ſo perfect.

THE RESISTANCE OF SOLID NON-CONDUCTORS.

I muſt refer you to the circumſtances of a fact already deſcribed, for the deſign of the ſeveral parts belonging to the following inſtrument.*

FIG. XLIII.

ABCD is a cylindrical glaſs veſſel, two inches in diameter and three in height. It is open at both ends, and filled with pitch. EF is a wire extending through the axis of the cylinder. Its ſituation is adjuſted while the pitch is hot. Its point F is even with the bottom of the cylinder. The body, through which the charge is to be conveyed, is applied to the point F, and faſtened in that ſituation by expoſing the lower ſurface of the pitch to the fire, till it begins to melt, and by placing upon it, while in that ſtate, one ſurface of the body carefully dried. If EF, when thus circumſtanced, be introduced into the circuit, the exploſion will paſs through ſuch a thickneſs of the body faſtened to the point, as will be commenſurate to its reſiſtance or non-conducting capacity.

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* Vol. I. p. 134.

A repreſents the thickneſs of the non-conducting body, which the ſame charge would perforate as was neceſſary, when two circuits were open, to ſtrike through B, which repreſents the correſpondent interval in air.

SHELL LAC.

A	B
$\frac{2}{20}$	3
$\frac{3}{20}$	No charge would paſs through this interval of the ſhell lac.

PLATE GLASS.

A	B
$\frac{1}{20}$	$\frac{5}{8}$
$\frac{2}{20}$	2
$\frac{3}{20}$	This thickneſs was ſtruck through by the higheſt charge which the jar would bear.

SULPHUR.

A	B
$\frac{3}{20}$	$1\frac{1}{8}$
$\frac{6}{20}$	$3\frac{3}{8}$

BEES-WAX.

A	B
$\frac{4}{20}$	$1\frac{1}{8}$
$\frac{6}{20}$	$2\frac{7}{8}$

OBSERVATIONS.

1ſt, It appears, from the preceding experiments, that ſhell lac is, of all non-conducting ſubſtances, that whoſe reſiſtance is the greateſt. If we add to this property, that of attracting moiſture with much leſs avidity than glaſs, we ſhall prefer it on all occaſions as the beſt ingredient for inſulation.

2dly, I have found ſome difference in the different ſpecimens of glaſs which I have tried, but not ſuch as I could meaſure with any accuracy.

3dly, In my experiments on ſulphur, I was often deceived by internal cavities which took place during its cryſtallization. I got rid of theſe by pouring the melted ſulphur into a mould, and by keeping a weight on its ſurface while cooling. There is one deſideratum yet remaining with regard to ſulphur, which is connected with the effect of water on bodies, when it becomes latent in their compoſition. When ſulphur is melted, its fluidity is remarkably great, owing to its water of cryſtallization. If the heat be continued, the water evaporates, and the ſulphur aſſumes a ſolidity which, in the moſt conſiderable fire, is retained till the ſulphur is diſperſed. I have tried in vain to examine its conducting power whilſt in this ſtate; for I have hitherto diſcovered no method of excluding the aqueous particles while the ſulphur is cooling. Indeed, as ſoon as it reaches a certain temperature, it abſorbs moiſture from whatever is in contact with it, becomes equally liquid with water itſelf, and then cryſtallizes.

4thly, You will obſerve, that in all the preceding inſtances, the appearances attending the electric fluid in its paſſage are invariably the ſame. The non-conducting body is torn or broken. The exploſion is luminous at the part where the fracture takes place, and no reſidue is left in the jar. In ſhort, every circumſtance is a proof that bodies, capable of excitation, are altogether impermeable.

The Reſiſtance of thoſe Subſtances which are partial Non-Conductors.

I mean, by partial non-conductors, thoſe bodies whoſe reſiſtance to the fluid is not that of *inſulating* ſubſtances, but ſuch as will admit a ſmall portion of a certain charge to paſs thro' them, and yet diſcover a fracture or ſeparation of parts whenever they are permeated by the whole charge.

LIGNUM VITÆ, OR GUIACUM WOOD.

This is the beſt of the partial non-conductors. It does not inſulate, but it reſiſts the higheſt poſſible charge when its thickneſs equals three-eighths of an inch. Before it acquired the property of inſulation, it required as much baking as thoſe ſoft woods, in which the greateſt ſtriking diſtance of the whole exploſion was double the length of its own.

I aſcribe this peculiarity to the greater difficulty attending the extrication of moiſture from the gum which it contains, rather than from the looſe fibres in which it is contained in other woods.

I found it ſcarcely poſſible to aſcertain, with any accuracy, the reſiſtance of wood when baked, as the fire, when it expelled the moiſture, left ſo many little cells and fiſſures as neceſſarily, in different experiments, to cauſe great varieties in the ſtriking diſtances.

A great change in this property is to be expected from the circumſtances which diſtinguiſh *charcoal* from baked wood. In the baked wood all the gum, or a great part of it, ſtill exiſts. In the charcoal, whatever is capable of evaporation, is burnt, and thoſe ingredients, on which the tenacity of the wood chiefly depends, are entirely removed: conſequently, charcoal muſt be a ſubſtance perforated by an infinite number of indefinitely ſmall cells or cavities, which, to the paſſage of a fluid equally ſubtile with that of electricity, muſt be much more favourable than the ſame ſubſtance when its cells are filled with non-conducting ingredients.

Dr. Prieſtley, and ſome other eminent electricians, have conſidered the conducting power of charcoal as nearly equal to that of the metals. There are circumſtances in which it approximates to this equality, and theſe were the circumſtances of their experiments; for they generally took the length of a ſpark, which ſtruck into a piece of charcoal, filed ſmoothly, and applied by the hand to a common conductor. Their eſtimate, therefore, was formed partially, rather than inaccurately: for you will now ſee that charcoal is not near ſo good a conductor as the acids in moſt caſes; in ſome caſes it ſurpaſſes them, but in all caſes it is much inferior in this reſpect to the metals.

EXPERIMENTS.

I filled tubes of different diameters with charcoal finely powdered and well dried. I then compared the lengths of the ſtriking diſtances when a given charge paſſed through the charcoal rather than through a metallic circuit, with the various intervals of air which interrupted the metallic circuit. A repreſents the ſtriking diſtance through charcoal; B, the interval of air in the metallic circuit. Tube, one-half an inch in diameter; diſtance of the conducting points in the powdered charcoal, four inches.

A	B
0	1
$1\frac{1}{2}$	$1\frac{1}{4}$
$1\frac{7}{8}$	2
$2\frac{7}{8}$	3

When one circuit only was

open, the greateſt ſtriking diſtance of the higheſt charge, the glaſs would bear through the charcoal, was four inches and a quarter; through the metal, four inches and three quarters. Tube, one-fourth of an inch; diſtance of the conducting points in the charcoal, four inches.

A	B
0	1½
1	1⅞
1½	2

The greateſt ſtriking diſtances through the charcoal, of the charge uſed in the preceding experiments, was two inches five-eighths; thro' the metal, three inches.

It is to be obſerved, that when the conducting points were brought within an inch of each other in the charcoal, the charge paſſed luminouſly through the ſubſtance of the coal, and then ſtruck through an interval of air in the ſame circuit, with as much brilliancy and ſound nearly, as if the whole communication had been perfectly metallic. The interval of air, which interrupted the circuit in this experiment was one-eighth leſs than it would have been if the charcoal had been removed.

The ſame varieties of reſult took place when the exploſion was conveyed over a ſtrip of powdered charcoal, half an inch acroſs, and one-eighth of an inch deep, extended over a dry ſurface of glaſs. The colour became leſs and leſs vivid, and the ſound weaker and weaker, as the ſtrip was lengthened. When the diſtance between the conducting points was ſeventeen inches, the whole ſtrip appeared brilliantly yellow when the exploſion paſſed over it. When the conducting points were ſeparated from each other by a diſtance of twenty-two inches, the light of the exploſion was a very faint yellow, and the ſound of it ſcarcely perceptible.

The following experiments were deſigned to ſhew the difference between the conducting power of charcoal and the vitriolic acid.

A repreſents the length of the

ſtriking diſtance through the acid, and B, through charcoal, when two circuits were open. The diameter of the tubes, containing the two ingredients, was one-fourth of an inch. The diſtance of the conducting points, four inches.

A	B
$1\frac{1}{4}$	1
$1\frac{1}{2}$	$1\frac{1}{4}$
$1\frac{3}{4}$	$1\frac{1}{2}$
2	$1\frac{3}{4}$
$2\frac{1}{2}$	$2\frac{1}{2}$

At all the above-mentioned diſtances the fluid divided itſelf into two ſtreams; but that which paſſed through the acid was by far the moſt loud and brilliant.

The earths, alkalis, calces of

metals, earthy, alkaline, and metallic neutral ſalts, are all partial non-conductors; but I have not yet found that any one of them, how much ſoever dried, was an inſulator. The varieties of their luminous ſtriking diſtances are conſiderable; but the ſhock, in paſſing through the tubes that contain them, never breaks the glaſs.

The diverſities are remarkable, which take place in the conducting power of water, when ſaturated with the neutral ſalts.

Saturated ſolutions of nitre, ſea ſalt, and ſal ammoniac, conduct the charge as well as if the water had been mixed with half its weight of vitriolic acid.

The conducting power of water is much improved, when charged with vitriolated iron, or with muriated or acetated veg alkali. But the property, as diſcovered in theſe ſolutions, does not approach the degree of it obſervable in the preceding. When the alkalis, earths, or calces; when cream of tartar, or aerial acid, is mixed with water, I cannot diſcover that its conducting power is in any reſpect changed.

The ſuperior effect of ſea ſalt, nitre, &c. may, I think, be cauſed by that ſuperabundance of acid which is generally found in their ſaline combinations, and from that diminution of attachment to the alkaline baſis, which, I have reaſon to believe, in many inſtances, attends the ſimple addition of water.

An incapacity of inſulation is the peculiar circumſtance by which I have diſtinguiſhed *partial* from *perfect* non-conductors. But I am wrong in this diſtinction, if Signor Volta be right, who aſſerts, that theſe partial non-conductors, in certain caſes, are *better inſulators than glaſs and other perfect electrics.* His aſſertion is ſupported by the following experiment.

If to a metallic diſk, ſuſpended by three ſilken cords, and charged, the equal ſurfaces of other diſks, uninſulated, and conſiſting either of marble, dry wood, wax thinly ſpread, be applied, it is obſervable,

1ſt, That on ſeparating the diſks, after they had been ſeveral hours in contact, a charge was always found in the metallic diſk. It is hence clear, he thinks, that the marble, wood, &c. acted as inſulators, though for ſo long a time in perfect communication with the ground.

2dly, If the partial non-conductors, uſed in the preceding experiment, were inſulated by ſtems of glaſs or ſealing-wax, the metallic diſk, after a few minutes contact, was found, on ſeparation, to ſhew no ſigns of electricity.

3dly, If the diſks, applied to

the metallic diſk, were made of glaſs or any perfect electric, the effect was exactly ſimilar to that of inſulating the partial non-conductors.

The two laſt circumſtances ſhould, I think, have induced Signor Volta to conſider the principal peculiarity of the experiment as dependent on ſomething which is altogether unconnected with the common cauſe of inſulation; for if he infers juſtly from the appearances of the fact, that wood, marble, &c. is an *inſulator*, another perſon might, with equal juſtice, infer that glaſs is a *conductor*, for it evidently diſperſes in the experiments, what the other ſubſtances retain. Signor Volta gives to the whole the name of *elec-*

trical paradoxes, and to protect the myſtical character with which he has inveſted them, he labours to prove that all the appearances are different from thoſe of his own electrophorus. I have frequently repeated the preceding experiments and have obſerved ſome circumſtances which ſeem to have eſcaped Signor Volta's attention.

1ſt, The deſcribed appearances never took place 'till the ſurface of the wood was almoſt baked, or 'till it was covered over with a thin plate of bees-wax. In this ſtate I cannot poſſibly diſcover the difference of the whole apparatus, from a common electrophorus not perfectly made.

2dly, If marble were uſed, to obtain ſigns of electricity, when it was ſeparated from its conjunction with the metallic diſk, it was not previouſly neceſſary to charge the diſk. In this caſe, then, the electricity that appears is produced, and is evidently the effect, not of failing to paſs through the marble as an inſulator, but of the conjunction of two different ſubſtances.

3dly, If, while the two diſks are united, a metallic connexion be formed between the metallic diſk and any part of the marble that is not touched by the metal, no ſpark will appear after the ſeparation. In other words, the equillibrium that was deſtroyed between the upper ſurface and the internal parts of the partial non-conductors is thus reſtored, ſimilarly to what takes place both in the electrophorus and the Leyden jar.

The mode of operation in the whole experiment I conceive to be the following:

When the partial non-conductors and the metallic diſk are brought into contact, they are able to retain more of the electric fluid, than when they are ſeparate. The fluid therefore that previouſly ſurrounded the diſk, is thus attached by the activity of a new attractive force, and conſequently cannot paſs forward through the adjoining partial conductors, whoſe reſiſtance certainly aſſiſts the attractive power.

But when the two bodies are ſeparated, the attraction is done away, and the fluid is again in the ſtate in which it was previous to the conjunction. If glaſs or any electric were uſed, the charge of the diſk is not adequate to the excitation, excepting when very thin; conſequently, no new attractive force takes place, and the electric fluid that ſurrounds the diſk is ſoon carried away by the various conducting ſubſtances that perpetually float in the air.

The reaſon is obvious why no ſecond ſpark is the conſequence of a ſecond contact. The fluid that ſurrounds the diſk after it is charged, is, agreeably to ſome principles which I have already illuſtrated, the cauſe of the new attraction that is formed. When that fluid, therefore, is taken away after the ſeparation of the diſks, a ſecond conjunction of them is not attended with the moſt active circumſtance that operated in the firſt: conſequently, a ſecond conjunction can be attended with no ſigns of electricity till one of the diſks is made a perfect electric; then the contact is of itſelf without any acid from communication, ſufficient to produce electricity.

Signor Volta's condenſer was produced by his attention to the circumſtances of the preceding experiment.

When the two diſks are in contact, ſcarcely any ſigns of electricity are perceptible. A vigorous ſpark from the metallic diſk is the invariable conſequence of their ſeparation. It is to be particularly obſerved, that this vigorous ſpark may be taken, when no other electricity has been applied to the diſks than that of a Leyden phial *almoſt ſpent*, or whoſe charge is ſcarcely perceptible. It is therefore concluded, that by a *ſmall quantity of the fluid, applied as in the preceding caſe, a much larger is collected or condenſed on the diſk.* The importance of the fact I do not deny; but I am convinced that it is improperly deſcribed, either as a condenſation, or as an increaſed collection of the electric fluid. The fallacy has proceeded from not diſtinguiſhing between the different appearances of the *ſame quantity* of fluid, in paſſing from the ſurface of glaſs and from a body of air. In the one caſe it is a ſmall ſtream, conſiſting of a rapid ſucceſſion of very ſmall ſparks; in the other, the whole quantity ſtrikes at once, and, when judged of by the ſight or the ear, is improperly magnified. But to prove that the ſpark does not conſiſt of more than what actually paſſed from the jar to the diſk, nothing more is neceſſary than to convey the ſpark back again into the jar; it will then re-aſſume its firſt inconſiderable appearance. Nay, a ſpark from a conductor fully charged, eight feet long and ten inches in diameter, when thrown upon the leaſt ſurface of glaſs, by applying a diſcharging-rod, is found to be ſcarcely ſenſible; but in caſe it were brought again into the circumſtances in which it was previous to its union with the glaſs, or if it were again diffuſed through a body of air, it would paſs into any conducting body in the form of a vigorous ſpark.

THE METALS.

The ſuperiority of the conducting power of all metals above that of all other bodies, is eaſily inferred and eſtimated from a great number of the experiments which we have already ſhewn and examined.---- The ſtriking diſtances and the expanſive power of the charge, in its paſſage through water, ſpirits of wine, &c. have been invariably compared with the ſtriking diſtances and the expanſive power of the *ſame* charge when it preferred a metallic circuit.

The conducting powers of *different* metals vary ſo inconſiderably from each other, that no minute and precarious diverſity of reſult, obſervable in experiments made on ſmall quantities of theſe bodies, can be attended to with any rational confidence.

As far as the ear and the eye can judge, the ſound of the exploſion and the brilliancy of the ſpark is equally great, whether it paſs through lead, iron, copper, or mercury, &c.

If different dimenſions of the *ſame* metal were placed contemporaneouſly in two circuits, ſo that the quantity of one might bear the proportion of 20. 30. or 40. to 1. when compared with that of the other, there was no obſervable diverſity in their ſtriking diſtances.

My experiments were made upon mercury; one portion of which I placed in the fineſt capillary tube, the other in a tube one-fourth or half an inch in diameter; but the exploſion would always paſs thro' both with equal eaſe, though I varied the ſtriking diſtances from half an inch in each-circuit to four inches.

The following facts, however,

ſhew that, notwithſtanding the amazing ſuperiority of the metals as conductors, the electric fluid, in its paſſage through them, meets with reſiſtance, which reſiſtance is conſiderable when the metals are particularly circumſtanced.

1ſt, In one of the two circuits I placed twelve yards of iron wire, one-twenty-fifth of an inch in diameter, and ſtretched out to its full length; in the other I placed an iron wire, one foot long and one-fifth of an inch in diameter. A given charge would rather paſs through the thick wire and ſeven-eighths of an inch in air, than through the ſmall wire, though interrupted by no reſiſtance but that which exiſted in itſelf. If the ſmall wire were coiled up, and placed in the circuit, the preceding difference was not perceptible.

2d, I placed five yards of braſs wire, one-fiftieth of an in diameter, and ſtretched out to its full length, in one circuit; in the other I placed one foot of iron wire, one-fifth of an inch in diameter. A given charge would rather paſs through the latter and three-fourths of an inch in air, than through the former.

Should circumſtances and your inclinations be favourable to the further inveſtigation of this ſubject, I think that a deciſive method for eſtimating the difference of conducting power in different metals, is ſuggeſted by the preceding facts. Nothing appears neceſſary but to place great lengths of very ſmall wires in two diſtinct circuits. I ſhould never uſe a leſs extent than twenty or thirty yards; nor ſhould the dimenſions of my wire ever exceed one-fiftieth of an inch. In this caſe I have no doubt that very conſiderable variteies would be obſervable, not only in the ſtriking diſtances, but in the expanſive force of any given charge.

EFFECTS OF RAREFACTION ON THE CONDUCTING POWERS OF BODIES.

The reſiſtance of all ſubſtances to the paſſage of the electric fluid increaſes with the diſtance to which their integrant particles are ſeparated from each other by the removal of preſſure, or by the influence of any repellent power.

When water, ſpirits of wine, or æther, is thrown into a torricellian vacuum, its ſeveral parts repel each other; they aſſume an aerial form, and, in their relation to the electric exploſion, differ little from air in the ſame circumſtances. In like manner, when the acids are changed from their fluid into a gaſeous ſtate, by mixing them with inflammable bodies, their conducting power ſeems to be loſt; for they will not admit the electric fluid to paſs through them, otherwiſe than luminouſly, and their ſtriking diſtances in ſeveral caſes are leſs than thoſe of air; though

I have little doubt in my own mind, yet I cannot aſſert experimentally, that they are *inſulators* in this ſtate; for it is ſcarcely poſſible to prevent a ſmall condenſation of their acid particles on the ſides of the veſſels containing them, and the leaſt condenſation of this kind acts very powerfully in a gradual diſcharge of the electric fluid.

It is a common notion amongſt electricians, that a *partial* vacuum is a good conductor. This notion is expreſſed in very *general* language. It is, however, not only inaccurate, but altogether falſe.--- Indeed, the following experiments ſeem to prove that the nearer you approach to a perfect vacuum, or the further you ſeparate the particles of any given matter from each other, the greater is their reſiſtance.

I filled a tube, forty inches long, with clean mercury. When this tube was inverted, the vacuum in its upper part was twelve inches long, in which nothing capable of reſiſtance to the electric fluid could exiſt, but the vapour formed by whatever air or damp might have been accidentally mixed with the mercury.

A given charge, however, would rather paſs through half an inch length in the open air, than thro' this partial vacuum. The terminations of the conducting wires were the ſame in both circuits.

I conveyed as much air into the above-mentioned vacuum, as filled an inch meaſure of the tube; but this quantity of air, thus expanded, reſiſted the charge more than twice the ſame quantity in its common ſtate of condenſation. When two inch meaſures were thrown into the vacuum, they reſiſted more than three and a half in their common ſtate.

You will obſerve, that every circumſtance in the preceding experiments was moſt favourable to the paſſage of the fluid through the partial vacuum. Such particularly was the damp on the ſide of the glaſs, and the removal of every influence which invites the charge to ſpend ſome of its force in a lateral direction.

I think we may with ſafety infer, that the reſiſtance would gradually increaſe as we approximated to that perfection of a vacuum, which my brother has proved to be altogether impermeable to the electric charge.

The following is an account of his experiment.

FIG. XLIV.

B is a tube fifteen inches long, filled with mercury, which has been carefully and accurately boiled in the tube till all air and moiſture are entirely expelled. One end is hermetically ſealed, the other inverted in mercury in the ciſtern H. The tube is kept erect by the braſs cap D, through a perforation, in which it paſſes at E. Five inches of the tube, meaſuring from the upper end A, is covered with tin foil. C is a valve adapted to the braſs cap, ſo as to be connected eaſily with an air pump. The ſmall iron wire, F, forms a communication between the mercury in the ciſtern and the braſs cap. All parts of this apparatus are cemented together, and thus rendered perfectly air-tight. When the cavity of the ciſtern, H, is completely exhauſted, the mercury (if well boiled) does not immediately fall. Its adheſion to the glaſs is ſuch as to require a conſiderable conquaſſation before it is overcome.

When the vacuum is thus formed, the coated part of the tube is applied to a charged conductor; and if it contains any medium capable of carrying off the fluid, its paſſage from the inſide of the tube will be rendered ſenſible by the appearance of light, and after this effect has continued for ſome ſeconds, a ſhock will be felt by connecting the tin foil with the braſs cap. But during the excitation of the ſtrongeſt machine, not a particle of light is obſervable in the tube, and the whole is as free from all ſigns of electricity, as if every part of the apparatus had been moſt perfectly inſulated.

Let us next fill the tube with unboiled mercury, and place it in ſimilar circumſtances; it is now obvious that a green light paſſes thro' the tube, and that the charge, communicated to its oppoſite ſides, is ſo great as at length to burſt through the glaſs.

I ſhould like much to vary the preceding experiment in the following manner:

Inſtead of a ſtrait, I would uſe a crooked tube, whoſe perpendicular altitude ſhould not be leſs than thirty inches. A vacuum, formed after boiling the mercury in ſuch a tube, would at each extremity be terminated by a conducting body, and we ſhould be able to diſcover whether an electric charge would paſs through *any length* of a perfect vacuum, or whether the ſuperabundance on one ſide of a charged glaſs could, with the aid of any medium, be conveyed to the other. In the Leyden phial, the attracting force is at a diſtance from the particles that are affected; and I cannot ſee why it ſhould not act as far as a certain limit through a vacuum as well as through an intervening electric.

A THEORETICAL APPLICATION OF THE DOCTRINE OF ATTRACTION TO THE DIVERSITIES OF POWER BY WHICH DIFFERENT BODIES CONDUCT THE ELECTRIC FLUID.

FIG. XLV.

AB repreſents a ſurface deprived of a certain portion of the electric fluid; CD, the correſpondent ſurface charged poſitively; EFGH repreſents a ſubſtance connecting the two ſurfaces, and poſſeſſed of its natural ſhare of the electric fluid.

It is obvious that AB, when a diſcharge is made, is ſupplied with what it wants from the ſuperabundance of CD, which is removed from one ſituation to the other, either by the attractive power of EFGH, or by the removal of a quantity equal to itſelf from the natural ſhare of EFGH, and then, by ſupplying its place, what is removed from the natural ſhare puſhes forward the next portion to it, and a ſucceſſion of removals thus takes place till the equilibrium is reſtored. This is the mode of operation which I have reaſoned upon in every part of theſe Lectures, though it might be eaſily ſhewn that the reaſoning I have uſed would be equally accurate, if the idea had been purſued, that the very quantity which is collected on the poſitive ſurface of a Leyden phial is conveyed by conducting media to the negative ſurface.

There are ſeveral contingencies poſſible to EFGH, a due attention to which will enable us to account for ſeveral varieties obſervable in the preceding experiments.

CASE I.

Let us ſuppoſe EFGH to form a line or a very narrow cylinder of electrical particles, equal to the quantity of the ſuperabundance on CD.

1ſt, It will hence be clear, that the whole line or cylinder muſt be removed before the charge can paſs. It is likewiſe clear, that the attraction, by which the particles adhere to EFGH, muſt be leſs than that ſubſiſting between the poſitive and negative ſurfaces; but the conſequence of removing a whole component from any ſubſtance, is perfect decompoſition. When, therefore, the paſſage of the electric fluid is attended with perfect decompoſition, we may, I think, aſcribe the effect to the preceding combination of circumſtances.

2dly, It is clear, that if the attraction of EFGH be ſuch as to retain *part* only of the fluid belonging to it, as its natural ſhare, the charge that paſſes will be ſmall in proportion to the magnitude of that part.

3dly, The momentum with which one particle puſhes away the next to it, is diminiſhed as that attractive force increaſes, by which each diſtinct particle adheres to its baſis. When a ſurface of one kind is placed on a ſurface of a different kind of matter, the effect of a projectile power, in moving either of the ſurfaces, will be regulated entirely by the adheſion of their ſeveral parts to each other. Do we not hence ſafely infer the reaſon why the expanſive force, produced by a charge paſſing through one body, is much leſs than that of the ſame charge paſſing through another which is a worſe conductor?

CASE II.

Let us now ſuppoſe that the ſpace, expreſſed by EFGH, contains much more fluid than the ſuperabundance accumulated at CD, or that the particles lying within a certain *given portion* of it, when removed, will part with enough to ſupply the deficiency: it is hence clear,

1ſt, That to produce the entire removal of the quantity in EFGH, a conſiderable increaſe muſt be added to the ſuperabundance which we have ſuppoſed to exiſt in CD. The reaſon, therefore, is obvious why certain dimenſions of one metallic body convey without fuſion that charge which melts the ſame dimenſions of another, and why the ſame effects in all metals are produced only by varying that quantity of electric fluid, to whoſe influence they are expoſed.

2dly, If the quantity and the attraction of the particles in EFGH be much greater than that of the ſuperabundance in CD, and of the force by which it tends to reſtore the equilibrium, then the charge will either not paſs at all, or its paſſage will be that of the fluid through a partial and feeble conductor, till ſome means are contrived to make its force greater than that of EFGH. Hence, when the conducting points are within a certain diſtance of each other in æther, ſpirits of wine, and water, low charges are conveyed through them weakly and in ſmall quantities; but high charges paſs thro' them completely and with full vigour. In other words, that reſiſtance, which two jars, charged to the height of fifteen grains repulſion, cannot overcome, gives way very eaſily to one jar charged to thirty grains repulſion.

3dly, To me the following appears to be the property of electric attraction.

When half the quantity of its natural ſhare of the fluid is taken from any given ſubſtance, what is left is retained with the force which at firſt attached the whole; or when one-half the number of particles is taken away, each remaining particle adheres to its baſis with double its original tenacity. This ſeems analogous to what takes place in chemiſtry; in which ſcience it is the language of all experience, that as the point of ſaturation is approached, the *difficulty* increaſes of uniting one ingredient with another; and, vice verſa, the laſt portion of ſeparable ingredients is with the greateſt difficulty ſeparated.

Admitting the principle I have advanced to be true, we ſhall ſee the reaſon, I think, why, in many caſes, that portion of fluid which reſiſts the power of a ſingle jar, is not to be moved by an endleſs multiplication of ſimilar jars equally charged.

Thus ſuppoſe the quantity and adheſion of the fluid in EFGH, to be equal 20. the contents of a ſingle jar to be equal 10. or ſuch as to be kept in equilibrio by half that reſiſtance in EFGH, which is expreſſed by 20. the addition of a ſecond jar would be equal only to 5. of a third jar, to 2½. of a fourth, to 1¼. Nay, an infinite number of jars would find a reſiſtance adequate to their force in each term of an infinite ſeries, the ſum of which equals the whole force of EFGH.

CASE III.

Let us now ſuppoſe that any ſmall portion of EFGH contains a quantity of fluid equal to the ſuperabundance of CD, it follows from this ſuppoſition, that the difcharge takes place when that ſmall portion is removed ſo as to take place of the next in ſucceſſion, and the next again of what ſucceeds till you come to the termination of EFGH, and that the force of the ſuperabundance at CD, towards its correſpondent deficiency, muſt be greater than the attraction of all the ſucceſſive portions of EFGH to their baſis.

1ſt, If the attraction be not infinitely ſmall, the difficulty of ſending the charge thro' EFGH muſt increaſe as EFGH lengthens. This is the caſe with charcoal and other bodies which we have examined.

2dly, If the attraction to EFGH be that which, when part of the ſmall portion is forced off, the remainder is operated upon with the power which at firſt attached the whole portion; then to facilitate the paſſage of the charge, it will be neceſſary to enlarge that part of EFGH which is firſt acted upon.

FIG. XLVI.

If, in the line EF, there are 16 particles, and a force as five will drive off four, but a force as 25 is neceſſary to drive off eight; in ſuch a caſe, to make a force as ten drive off eight particles, the action muſt be directed againſt double the length of EF; for then the additional four come off with as much eaſe as the firſt four from EF.

This is the caſe with water, ſpirit of wine, &c. for when double the force will very little increaſe the charge paſſing through a tube almoſt capillary, by enlarging the diameter of the tube, the charge is conveyed thro' it with the greateſt eaſe.

3dly, Let us ſuppoſe the charge adequate to the removal of particles meaſured by any given part of EFGH, by introducing a body which yields its natural ſhare to the momentum of the charge eaſier than EFGH, we leſſen the force neceſſary to the diſcharge. Hence it is evident that when acid is mixed with water, or water with ſpirits of wine, the ſtriking diſtance is increaſed, and every circumſtance expreſſive of a more eaſy paſſage takes place.

CASE IV.

Let us now ſuppoſe that the electric fluid belonging to EFGH cannot be removed without a contemporaneous motion in its other integrant particles; in ſuch a caſe, it is obvious that the charge, previouſly to its paſſage, will have two powers to conquer; that which attaches its natural ſhare of the electric fluid to EFGH, and that by which the component parts of EFGH adhere to each other.--- Hence, when the charge is ſent through air, oil, water, &c. in certain circumſtances, an expanſion takes place, capable of producing very conſiderable but various degrees of effect. The peculiarities obſervable in theſe facts are what may be fairly inferred from the nature of the cauſe which operates.

1ſt, Let us ſuppoſe that a ball, D, is moved by any given force, and with its received motion is to ſtrike another ball, E; if D be impeded by no friction, or be adherent to no other ſubſtance, then it will impreſs E with the whole force it received; but if it adheres to any other body, then it will ſtrike E with a force diminiſhed in proportion to the ſtrength of the adherence. Hence we find that the particles of air expand much more forcibly than particles of oil; for the latter are united with a tenacity to each other, which does not take place amongſt the former.

2dly, In proportion to the degree of adherence which impedes the removal of D, ſo will the number of ſuch particles be as are capable of being removed. Hence we find, that where the expanſion is greateſt, owing to the mobility of the conducting medium, there the length of the ſpace, through which the charge paſſes, is greateſt; that charge which paſſes through five inches in air, will paſs through three-eighths of an inch only in oil. I make a compariſon between oil and air; for they are the only ſubſtances which conduct no charge whatever without expanſion. This is not the caſe with æther, ſpirits of wine, water, &c. A partial diſcharge may be conveyed thro' them without any ſenſible motion, and in conſequence of this peculiarity, the expanſion, produced when the charge paſſes through the *luminous* diſtance, is diminiſhed, ſo that the force is affected by a cauſe which does not operate in the preceding inſtances.

A THEORETICAL EXPLANATION OF THE METHOD, ACCORDING TO WHICH THE ELECTRIC FLUID OPERATES, WHEN GLASS AND OTHER SOLID ELECTRICS ARE BROKEN BY THE EXPLOSION.

I have already informed you, that when the charge is conveyed from one point to another, placed within a given diſtance in oil, water, &c. the glaſs tubes containing theſe fluids are always broken.—This effect is not peculiar to glaſs; it is equally certain upon ſealing-wax, roſin, ſulphur, and all *brittle* non-electrics. By the words *brittle* non-electrics, I mean ſuch as are capable of being cracked in conſequence of percuſſion; for when the electric is ſoft, like bees-wax a little warmed, a ſmall perforation is the only apparent ſeparation of parts which I have been able to diſcover.

It is difficult to determine what thickneſs, or whether any thickneſs in the ſides of the tubes, will protect them againſt this peculiar action of the charge in its paſſage. I have broken a column of glaſs, whoſe diameter was not leſs than three inches, and whoſe cavity, which contained the water, did not exceed one-fourth of an inch in diameter. I have frequently burſt a green glaſs bottle, in which the diſtance on each ſide, between the conducting points and the ſides of the glaſs, was not leſs than two inches and a half.

I took care, in all my experiments, that no preſſure confined the water; and I obſerved, that when the glaſs broke, not the leaſt expanſion took place in the water. ---Sometimes, however, the charge would paſs luminous without breaking the glaſs; *in that caſe there was a very ſtriking degree of expanſion in the whole body of the water.*

An explanation of the preceding is the eaſy conſequence of an attention to thoſe explanations of ſimilar facts, which have been given by ſome of our beſt philoſophers.

FIG. XLVII.

Let AB repreſent a lever delicately balanced upon the fulcrum

C; when the extremity A is ſtruck, its adherence to the ſeveral parts of the lever, which I have repreſented in the figure by little circles, will communicate its motion, by a ſucceſſion of infinitely ſmall portions of time, from one to the other till it pervades the whole; and when this takes place, then only does the lever turn round.

Now if the velocity, communicated by the percuſſion or blow, be ſo great as to remove the part or circle at the extremity, A, out of the ſphere of the attraction of the next particle before the motion can be communicated to all the parts of the lever, then the part A will be ſtruck off or ſeparated from its connection with the reſt of the lever, which of courſe will not be turned. What is thus deduced as a neceſſary conſequence, from the well known principles of motion, is clearly illuſtrated and proved by experiment.

If the lever, AB, be made of wood, a piſtol ball will ſtrike off a part of the wood equal to what it occupies in its paſſage, but will not in the leaſt affect the lever, though ſuſpended with the utmoſt delicacy.

A muſket ball will paſs through a board ſuſpended by the fineſt hair, without cauſing the leaſt change of poſition. In making theſe experiments, great care ſhould be taken that the perſon, who fires the piſtol or the muſket, be ſo far diſtant that the concuſſion of air, cauſed by the exploſion of the powder, ſhall not affect the board or the lever.

We may confidently aſſert, that the velocity of the action of the electric fluid is ſeveral thouſand times greater than that of the moſt potent gun-powder.

It is reaſonable, then, to ſuppoſe that when it paſſes through any part of a body, it may exerciſe its force on that part of the body which is immediately expoſed to the action of the fluid, much more rapidly than its force can be communicated to the whole body.

FIG. XLVIII.

Let ABCD repreſent a quantity of oil or water; let abcd be the part through which the electric fluid paſſes with a velocity infinitely great; the oil within the ſpace, abcd, will be moved juſt as the extremity of the lever was by the piſtol-ſhot, with a very great momentum, and, as we find by the experiment, no motion will be communicated to the parts above or below, but the momentum of the oil will be communicated to the particles of glaſs lying in the direction of their motion, and, agreeably to what takes in the oil, the particles of glaſs will be ſeparated before thoſe above or below them can be affected.

The greater or leſs *brittleneſs* of a body is nothing more than the greater or leſs force by which the parts of a *whole* body are kept together. The extremity of a glaſs rod is broken off eaſier than the extremity of a wooden or metallic rod, becauſe the particles of the *whole* rod are not ſo cloſely connected in the one caſe as in the other; conſequently, not ſo ready to diffuſe *generally* an effect applied *partially*.

What takes place as the conſequence of all other kinds of percuſſion, is exactly ſimilar to the effects of electricity. The wood muſt be very thin indeed, which is broken when placed in the ſituation of abcd. Suppoſe that, in this ſituation, a ſubſtance could be placed which would not give way at all, then the momentum muſt be generally diffuſed through the whole maſs of the oil: we conſequently find, that when abcd is ſurrounded by wood of a certain thickneſs, no fracture enſues, and the exploſion is followed by that general expanſion which ſeveral experiments, already ſhewn to you, were deſigned to meaſure.

THE APPLICATION OF ELECTRICITY TO THE APPEARANCES OF NATURE.

THUNDER.

It ſhall now be my endeavour to acquaint you with the progreſs made by philoſophers, in applying electricity to the ſtudy of nature. The advantage of the ſcience in this relation would appear moſt ſtrikingly, if I had time to amplify on the darkneſs which involved the ſubjects it has illuminated, or if I could minutely deſcribe thoſe theories whoſe departure is of a date ſo recent as to have left traces of their exiſtence in the memories and prejudices of many philoſophers ſtill living. We have no time, however, for expoſing the errors of our predeceſſors, and we ſhall better employ ourſelves, if, inſtead of ſquandering our attention upon miſtakes, we direct the whole force of it to the due examination of received truths.

It is remarkable, that the moſt *ſucceſsful* and *important* application of electricity to the appearance of nature was the *firſt*. The ſnapping noiſe of the ſpark engaged Mr. Grey's fancy, as having ſome ſimilarity to the noiſe of thunder.—Till the conjectures of the Abbe Nollet, however, we find nothing, either in the ſpeculations or purſuits of electricians, to favour Mr. Grey's firſt ideas. The Abbe's language is indeed very ſtriking, and reflects great credit upon his character as a theoriſt; but it gives him no portion of that diſtinguiſhed praiſe which Dr. Franklin has derived, as well from the mode of argumentation as from the experiments which have eſtabliſhed the immediate agency of the electric fluid in the production of thunder, lightning, and all their concomitant appearances. The firſt endeavours of this admirable philoſopher are deſigned to guard us againſt the conception of any difficulty from the amazing difference in *degree* which ſubſiſts between the effects of our electrical experiments and thoſe produced by the vaſt apparatus of nature. If our ears be ſtunned by the exploſion of a ſmall battery, the loudneſs of thunder cannot exceed what we ſhould hence reaſonably expect, when we ſuppoſe that in one caſe a few feet only are charged, and in the other a ſurface adequate, perhaps, to that of 10,000 acres. The reſemblance between the cauſe of thunder and the electric fluid is marked out by Dr. Franklin as conſiſting in a great number of leading particulars.

1ſt, The zig-zag appearance of lightning is exactly ſimilar to that of the electric ſpark or ſhock.

2d, Lightning ſtrikes ſuch objects as are high and prominent, viz. the ſummits of hills, the extremities of lofty branches, ſpires, the maſts of ſhips, &c. The electric fluid, when a ſimilar opportunity is offered, will always prefer ſtriking into a point, rather than into a blunted body.

3d, The ſame bodies, which are good or bad conductors of the electric fluid, are equally ſo of that which cauſes lightning.

4th, Lightning burns, ſo does the electric fluid.

5th, The ſame may be obſerved of the fuſion of metals.

6th, Thoſe bodies which are rent by the paſſage of the electric fluid, are affected in a ſimilar manner by lightning.

7th, Lightning has been known to ſtrike men with blindneſs. Dr. Franklin produced a ſimilar effect on a pigeon, by paſſing through it a large charge of electric fluid.

8th, Lightning deſtroys animal life, ſo does the electric charge.

9th, Both cauſes have the ſame power of making iron magnetic.

Few theories in natural philoſophy are ſupported by reaſons ſo convincing as thoſe which I have now enumerated. The Franklinian theory of lightning, though eſtabliſhed by them, is moreover demonſtrable without their concurrence; for the author of it contrived to empty the clouds of their ingredients, and to make theſe the ſubject of examination by our ſenſes. He raiſed a kite, which had an iron pointed rod fixed to it. He uſed a hempen cord, wetted; the extremity of this cord was faſtened to a key, and ſeparated from the hand by a ſilk handkerchief. The firſt electric appearance, obſerved by the aid of this apparatus, was the ſeparation or the erection of the looſe hempen threads which ſtuck to the cord. This circumſtance led him to apply his knuckle to the key, by which means he received a very evident electric ſpark, and ſoon after a very copious diſcharge of the electric fluid. By the aid of an inſulated conductor, faſtened to his houſe, he procured ſuch repetitions of the ſame facts as were fully ſatisfactory. Dr. Prieſtley has given a very entertaining hiſtory of this diſcovery. It well deſerves your attention; I ſhall therefore refer you to his work.*

Your minds, I ſhould ſuppoſe, can now retain no doubts about the cauſe of thunder and lightning, or that, ſome way or other, it is produced by the electric fluid. I will therefore proceed to the more particular application of the Franklinian theory to this ſubject; for when the agency of the electric fluid in the production of lightning is acknowledged, our next buſineſs ſhould be to illuſtrate the probable mode of that agency.

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* Hiſt. of Electricity, Vol. I. p. 217.

1ſt, We are furniſhed with experiments without number to prove, that during the time of a thunder ſtorm there is a contemporaneous accumulation and deficiency, or two parts in the atmoſphere that are in the oppoſite ſtates of poſitive and negative. By the aid of an inſulated conductor this proof is eaſily obtained; for ſuch a conductor will ſhew poſitive and negative ſigns, alternately, ſeveral times within the ſpace of an hour. This is purely the conſequence of motion in the clouds; for, during one inſtant, the poſitive cloud will be over your conductor, and when that has departed, its correſpondent negative cloud ſucceeds, and thus produces the deſcribed effect. But,

2dly, We muſt readily perceive the nature of the thunder clap, and its concomitant lightning, when we have once acknowledged what I have now endeavoured to prove; for whenever the accumulation and its correſpondent deficiency arrive to a certain height of charge, the equilibrium is reſtored by a ſudden exploſion, which reſembles in its circumſtances the ſpontaneous diſcharge of a Leyden phial: for in both caſes the fluid chooſes that channel which affords the eaſieſt paſſage, or is endowed with the greateſt ſhare of conducting power.

In my opinion, the greateſt diverſities of a thunder ſtorm proceed from this *ſingle circumſtance*: Nineteen out of twenty, nay, with ſafety I may aſſert that ninety-nine thunder-claps out of one hundred, are nothing more than the harmleſs diſcharge of one cloud into another. When thoſe rare inſtances happen, in which the accumulation ſtrikes into the earth, *I cannot poſſibly conceive the deficiency to be in the earth;* but in ſome other cloud to which an eaſier paſſage is found through ſo good a conductor as the wet earth, than through a body of air which is a perfect non-conductor.

I am ſingular in this ſuppoſition. The novelty of it requires that I ſhould amplify, and thus do juſtice to you as well as to myſelf.

It has hitherto been the language of electricians, that every diſcharge of the electric fluid, either from the clouds into the earth, or from the earth into the clouds, is nothing more than the diſcharge of a poſitive cloud to ſupply a deficiency exiſting ſomewhere or other in the earth, or of a ſuperabundance in the earth to ſupply a deficiency in the clouds. To this theory my objections are the following:

1ſt, We have not been furniſhed by artificial electricity with a ſingle fact, in which a *conducting ſubſtance* is capable either of taking a ſuperabundance, or of loſing a part of its natural ſhare. The coatings on both ſides of charged jars may be removed, and the quantity of electricity will ſtill remain undiſturbed. Conductors, ſuſpended in air, may be annihilated, and the air ſtill continue either in its poſitive or negative ſtate. If you ſuſpend an inſulated chain, and charge it, by dropping the chain into the cup, and then remove it, no electricity will be found in the cup, but the atmoſphere, which previouſly to the removal ſurrounded the ſame cup, will continue to be equally charged. —The ſuppoſitions of Lord Stanhope, and his account of the man who was ſlain by *thunder without light*, are directly contrary to what I regard as eſſential principles of this ſcience. But in philoſophy, we muſt bow to the authority of *facts only*, and of theſe I have not yet ſeen any in which the conducting ſubſtance itſelf, and not the air ſurrounding it, is capable either of a ſuperabundance or a deficiency. But,

2dly, In the preſent inſtance we may reaſon, not from experience only, but from theory alſo. The accumulation in one part of the body ſuppoſes an intermediate reſiſtance between it and the other part of the ſame body in which the deficiency is found, eg. As ſoon as the leaſt fiſſure takes place in the glaſs, the fluid immediately paſſes from one ſide to the other; or if an electric of any kind be covered with moiſture, it is well known that the greateſt power of artificial electricity is incapable of communicating any charge to it. How then can we ſuppoſe that a metallic or any other conducting ſubſtance, which by its nature precludes all poſſibility of the reſiſtance neceſſary in theſe caſes, ſhould be capable of the effect in queſtion, or of an electrical charge?

3dly, No man will doubt the conducting power of the earth; and if what has been juſt obſerved be true, how is it poſſible that it ſhould ſo far differ from other conducting ſubſtances as to admit of a correſponding poſitive and negative? But,

4thly, It is univerſally acknowledged by electricians, that no body can contain more than its natural ſhare of the electric fluid. How is the common theory to be reconciled to this principle? If it be aſſerted that the ſuperabundance is in the air, and the negative in the earth, then the air muſt have more than its natural ſhare of the fluid, and the earth leſs. Vice verſa, the ſame, according to this theory, may be obſerved of the earth and the air.

The four objections which I have now ſtated are oppoſed immediately to the ſuppoſition that the earth can be charged at all by itſelf, or in conjunction with the atmoſphere. The only imaginable hypotheſis muſt be derived from allowing the atmoſphere alone in this inſtance to be capable of an electric charge. Indeed, the reaſonableneſs of ſuch an hypotheſis cannot be denied. It is the neceſſary conſequence of the Franklinian theory, that if it ever have a ſuperabundance in one part, it muſt have a correſpondent deficiency in ſome other. Now, between theſe parts, let the earth be ſuppoſed to lie, and in conſequence of its being a good conductor, it muſt act as a diſcharging rod, and in this capacity will, I think, account for all the appearances of thunder in its paſſage to and from the earth. The cauſe aſſigned is ſuch as muſt act in *ſome* caſes; it is, I think, in *all* caſes adequate to the effects produced.

1ſt, Let the poſitive cloud be ſuſpended at ſome diſtance from the earth, and ſeparated from it by a body of air which is not charged with any of the fluid. Though the negative portion of the atmoſphere touched the earth in this caſe, the inſulation would, notwithſtanding, be perfect; and when the motion of the cloud leſſened its heighth above the earth, ſo as to bring it within the ſtriking diſtance, then a thunder clap would take place, and one flaſh of lightning only would appear; for as long as the negative portion lay on the ſurface, the communication of the ſuperabundance would be made thro' ſo many points of contact, as to prevent the appearance of one compact body of the fluid. An effect, ſimilar to what I have now obſerved, takes place in the diſcharge of the Leyden phial. If, when the rod forms the communication, it *touches* neither the outſide nor the inſide, then two ſparks will appear at the ſame inſtant; but if the rod ſhould touch the outſide, and only approach the inſide, then one ſpark only will appear.

2dly, If the poſitive cloud were

in contact with the earth, and the negative at ſome diſtance above it, then (for reaſons ſimilar to what I have aſſigned in the preceding caſe) the charge would appear to ſtrike into the negative only, and, agreeably to what takes place in nature, the thunder would actually paſs from the earth into the clouds.

3dly, If both clouds be inſulated, but not ſo as that their joint diſtances ſhall be greater than the ſtriking diſtance, in ſuch a caſe, two ſtrokes will appear at once, agreeably to what I have frequently obſerved in the courſe of my attention to theſe appearances.

4thly, If the eminences and pointed terminations, which are ſcattered over the ſurface of the earth, approach near enough to influence the poſitive cloud; if other eminences, and in a different part, are alike near to the negative cloud, the appearances, uſually ſhewn by elevated conductors, muſt take place, and theſe appearances muſt vary from poſitive to negative, as their motion will vary the poſition of the clouds, and conſequently their relative diſtances from the conductor.

I muſt obſerve here, that the caſe in which two different flaſhes of lightning appear contemporaneouſly in different parts of the heaven, are not eaſily explicable by the common theory, without introducing more cauſes than are requiſite for the aſſigned effect. To explain this appearance, electricians in general have ſuppoſed that a poſitive and negative are in the air, and at the ſame time another poſitive and negative in the earth; that the poſitive in the air ſtrikes into the negative in the earth, and the poſitive in the earth into the negative in the air. The objections I have made to the poſſibility of charging a conducting ſubſtance. operates ſtrongly here, and beſides, it is manifeſt, I believe, from the explanation I have given, that this complicate ſuppoſition is not at all neceſſary.

I would further obſerve, that the ſudden and frequent changes from poſitive to negative, ſhewn by elevated conductors, are wholly myſterious to thoſe who adopt the common theory: for if the correſpondent ſuperabundance or deficiency be in the earth, what becomes of it when the change takes place without any apparent diſcharge of the fluid in the form of thunder? A gradual diſcharge will not remove the difficulty.

As preſumptive evidences only, for the ſuppoſition that the earth acts as a diſcharging-rod, I ſhall enumerate a few circumſtances, which are conſidered and allowed to be general attendants of a thunder ſtorm.

1ſt, Damages are commonly done when the clouds are very low.

2d, Damages uſually take place when the earth is moiſtened, and thus rendered moſt powerful as a conductor.

3d, By comparing a great number of accounts of thunder ſtorms, two out of three nearly will be found to have happened near ſome great river, or near thoſe parts of the earth which afford the leaſt reſiſtance to the paſſage of the electric fluid.

Having fully explained to you my ideas of the manner in which the earth is expoſed to the dangers of lightning, I now proceed to a a third circumſtance obſervable amongſt the concomitants of thunder which deſerves inveſtigation, viz.

THE FORM OF THE FLASH.

This is various; the zigzag is ſometimes larger than at other times, and in many inſtances the current divides itſelf into ſeveral diſtinct ſtreams. The diverſities of the zigzag might be expected from the heterogenous nature of our atmoſphere, in which particles of matter, whoſe conducting power vary much in degree, are placed with no conſtant order. The fluid, therefore, in paſſing through the air, and in ſtriking to theſe particles, as to ſo many ſtepping-blocks irregularly placed, can ſeldom be ſeen twice ſucceſſively to obſerve the ſame track. The diviſion of the lightning into ſeveral diſtinct ſtreams, I have very well imitated by an experiment which I made in conſequence of conjecturing the cauſe on which it depends. You well know, that the effect of the charge which paſſes from the poſitive ſurface of a body to its negative, depends very much on the conducting power of that intermediate body which connects them. If a tube, one-fourth of an inch in diameter, filled with water, be employed as the diſcharging-rod of a very large battery, it is a very trifling effect that is produced; but if ſeveral of theſe tubes be uſed, a portion of the fluid will paſs through each tube in the form of a diſtinct ſtream to the outſide of the jar.

FIG. XLIX.

A is the repreſentation of a Leyden phial; ab, cd, ef, &c. are glaſs tubes one-fourth of an inch in diameter, and filled with water; they are terminated by metallic balls, which are at equal diſtances from other metallic balls, whoſe affixed wires, l, m, n, are connected by a chain with the outſide of the jar A. BC is a metallic rod terminated by two metallic balls, and turning round on an inſulated pivot, ſo that at the ſame inſtant, in which it touches E, it may likewiſe touch DG, which is connected with the inſide of all the tubes. You ſee that a complete circuit is thus formed, and upon trial, you may likewiſe obſerve, that the charge will paſs in ſeveral diſtinct ſtreams from a to l, from c to m, from e to n, and ſo on. In nature the effect is ſimilar to what I have now exemplified. The charge of the cloud finds in the earth ſeveral diſtinct paſſages at once, each of which carries its ſeparate portion, and as they terminate in different parts of the atmoſphere, muſt be the means of conveying the fluid in ſo many diſtinct directions to the deficiency or from the ſuperabundance. I have ſcarcely ever examined the devaſtations of a thunder-ſtorm, without ſeeing ſtriking evidences of its being divided through more currents than one. Some electricians, owing to their ignorance of what I have juſt explained, have been very much perplexed in tracing the direction of particular ſtrokes.

THE PECULIARITIES ATTENDING THE NOISE OF THUNDER.

All ſounds, which come from a conſiderable diſtance, ſtrike the ear with ſucceſſive reverberations. It is an effect which ſpreads in all directions, and there are but few objects from which it will not rebound; conſequently, theſe reboundings, as they come from objects more or leſs diſtant, muſt ſtrike the ear at different inſtants of time. The exploſion of a cannon, owing to this cauſe, if you be within one mile of it, after the firſt pounce, is always followed by a ſeries of other ſounds, which are leſs loud as they are later, or as the vibration is communicated from objects which are more and more diſtant.

The obſervations, which I have now made, may be accurately applied to ſuch peculiarities as attend the noiſe of thunder. The exploſion, at a little diſtance, is always attended with a grumbling hoarſeneſs or noiſe, whoſe violence gradually dies away. If the exploſion be very near, the craſh is inſtantaneous, and exactly ſimilar to what you hear when you are cloſe to the diſcharge of a cannon. If it be heard in a high and uneven country, where the objects capable of reverberating are numerous, the noiſe is that of a long and broken ſucceſſion of claps, which vary in loudneſs, not with the length of time, but with the nature and circumſtances of the reverberating objects. At ſea there is no diverſity in the reverberating bodies, and the ſeries of thunder-ſounds is regular, and decreaſes in loudneſs as the length of time increaſes.

I muſt not here omit the eaſy method by which you may know the diſtance of a thunder-ſtroke. By obſerving the flaſh, and by counting the moments which eſcape between the flaſh and the roar, you are furniſhed with ſufficient data for this purpoſe; for ſound travels at the rate of 186,768 feet in a minute: by reducing this portion of time into ſeconds, and allowing for each its proper number of feet, you get with accuracy the diſtance of the ſtroke in a direct line from your own ſituation. But the diſtance thus aſcertained, is by no means the meaſure of our removal from danger; for if what I have ſaid on this ſubject be admiſſible, it muſt appear that the diſcharge of the fluid into the earth may be very diſtant from the part in which it returns again to the clouds.

THE COLOURS OF THE FLASH.

The flaſh is various in its colours, and this variety depends very much on the denſity of that medium thro' which the light has paſſed. The air, at the time of a ſtorm, is generally thick, and it ſeldom happens that the light is ſo vivid as not to be gazed at without great inconvenience to the eyes. A ſtorm is lately ſaid to have happened, which killed a man and ſome horſes, without any appearance of light. I have carefully examined the evidences of this ſtory, and I feel no diſpoſition to regard it with the leaſt credulity. That two common men, a Scotch ſhepherd and a Scotch carter, who acknowledge themſelves to have been ſtunned at the time by the noiſe, and who were evidently frightened before they took any notice of the ſurrounding objects, ſhould not have ſeen the flaſh, whoſe roar firſt awoke them into the uſe of their eyes, is not half ſo wonderful as that ſeveral learned men ſhould regard the forgetfulneſs of ſuch terror and ſtupidity, as an evidence that *no flaſh could be ſeen.* I was once preſent at the examination of ſome poor perſons, who may be ſuppoſed equally reſpectable in their intellects with the carter and ſhepherd in queſtion, and who were on the ſpot when a houſe was fired by a thunder-ſtorm. ——The contradictory abſurdities which they aſſerted and maintained, are ſcarcely conceivable. Indeed, the human fancy at all times is abundantly prolific of deceits, but never equally ſo, as when it is embarraſſed, or ſtupified by terror; it then conjures up ſpectres it never ſaw, and is inſenſible to the impreſſions it might have retained.

THE CIRCUMSTANCES ATTENDING THE DEVASTATIONS OF LIGHTNING.

The ſtroke fuſes metals, and perfectly calcines them when their dimenſions are very ſmall. It renders iron magnetic, and gives braſs the ſame property, if it contain any ferruginous particles. The changing of a conductor's pointed end into a blunt end, is the greateſt fuſion which I have yet ſeen authenticated. I have heard of the fuſion of bells, of large chains, and of an iron conductor an inch in diameter; but the authority of theſe facts is not ſuch, in my opinion, as to deſerve your confidence. I have ſeen a knife, which part of a ſtroke had paſſed through, whoſe violence had demoliſhed a houſe; but it ſcarcely ſhewed any marks of fuſion, ſave on the edge and near the point. I have ſeen part of a bell-wire, which was ſhortened only by the diſcharge of a thunder-cloud; and we very well know, that the ſhortening of the wire is an effect produced by a leſs charge than is neceſſary to the fuſion of the ſame wire. By the aid of artificial electricity, we can reſemble Nature in all theſe effects of her violence. It is a ſmall battery that is ſufficient to melt a wire; nay, the addition of force neceſſary to calcine it completely is not very great.

The power of the electric fluid

to melt metallic bodies is not, in my opinion, to be aſcribed to any additional heat or light; both of which agents, it is well known, produce the ſame effect, and both of which, it is well known, are attendant upon theſe effects of lightning. That charge of a battery, which is adequate to the fuſion of metals, cannot warm the leaſt quantity of water, which would certainly be the caſe, if it communicated any portion of heat during its paſſage.

The increaſe of fluidity, in any caſe, is nothing more than the removal of the particles conſtituting any body to a greater diſtance from each other. Air is rendered more fluid by taking away the ſuperincumbent preſſure of the atmoſphere. A pint of air, when conveyed into a vacuum meaſuring a quart, will fill the whole of it, and its particles, therefore, will be removed to double their former diſtance from each other. Fluidity, moreover, is increaſed by adding a more to a leſs fluid body, as is the caſe when water and ſyrups are mixed, or ſpirit of wine and water. In like manner, metals derive their fluidity from the interpoſition of any new body; for its particles are thus ſeparated to a certain diſtance from each other.

I think the electric fluid is analagous in its effects to thoſe of the cauſe I have juſt explained, when a large portion of it is added to any body. If it be aſked, what the heat and the light are, which eſcape in theſe inſtances, I ſhould anſwer, that they are the conſtituent parts of the metal, which are now ſeparated, becauſe, when the component particles of the metal are removed to a greater diſtance from each other, their power of retaining what depends on an attractive force muſt be diminiſhed, and conſequently ſome of the ingredients muſt eſcape. However, I now point out to you what ſtrikes me as only likely to take place, and would by no means aſſert that the mode of operation is actually that by which the effects of lightning or the electric fluid are produced.

I would now proceed to in-form you, that the devaſtations of thunder are generally attended by the ſplitting of ſtones, and the combuſtion of wood and other bodies capable of being burnt. I have, in a variety of inſtances, produced effects analogous to the ſplitting of the ſtones.—Glaſs, flints, ſugar, and many other ſimilar bodies of ſome pounds weight, I have torn into very ſmall pieces, by introducing two wires into them, ſo as to approach very near to each other, and by placing them, thus circumſtanced, within the circuit of a ſingle phial. If any moiſture ſhould exiſt between the two points, the effect is much more ſtriking. Indeed, whatever ſolid body the electric fluid is incapable of melting in its paſſage, is always torn by it into ſmall fragments.

The ſudden and partial addition of heat to either of the abovementioned bodies is attended with ſimilar appearances, and, I ſhould ſuppoſe, is produced in a ſimilar manner, or by cauſing an expanſion, which removes the parts of the body to ſuch a diſtance as to take away the influence of that adheſive power on which their continuity depends.

The burning of combuſtibles, by the paſſage of the electric fluid, I would account for on the ſame principle as thoſe which I have applied to the fuſion of metals, viz. by ſhewing that the addition of a new body is attended with a decompoſition, and in my opinion combuſtion is a ſpecies of decompoſition or ſeparation of the conſtituent parts which form the burning body.

To the other devaſtations of lightning, we ſhould add the deſtruction of life, both in the human being and in all other animals.—The inſtantaneouſneſs of this effect, and the very rapid putrefaction which follows it, are very remarkable circumſtances.—The electric fluid makes no wounds, but either ſtuns or demoliſhes in a moment. If it ſtuns only, you awake very ſoon in perfect health, as if you awoke out of a profound ſleep. If it demoliſhes, the body muſt be buried very expeditiouſly, or it becomes perfectly rotten and offenſive. The body of profeſſor Richman, the firſt ſcientific victim to the rage of lightning, was ſo much corrupted within the ſpace of two days, as to be removed with difficulty to the coffin.

The extravaſated blood that is found in the bodies of thoſe who have been ſtruck by lightning, together with the rapid putrefaction of their whole ſubſtance, are ſtrong evidences that ſeveral of the blood veſſels are burſt, and that a general laceration of parts has taken place. But the preceding are not conſtant, they are not therefore the neceſſary effects of the electric fluid on the animal frame. In many caſes of the death cauſed by thunder, ſcarcely any mark of the injury is diſcoverable by the eyes, and I know not one inſtance in which the application of artificial electricity has been attended with a rupture of the blood veſſels.

An attention to the invariable conſequences of the electric ſhock, when directed ſeparately through different parts of the human body, will perhaps juſtify us in conſidering the nerves as thoſe parts which are firſt and moſt powerfully affected by its influence.

When the full charge of two ſquare feet of glaſs is conveyed through the head, it produces a momentary forgetfulneſs, which is followed by a depreſſion of ſpirits and a general proſtration of ſtrength. The patient is frequently afflicted with head-aches, and continues for ſome time in a trembling and irritable condition.

In correſpondence with the preceding, are the effects of conveying a battery through the head of a bird. Its optic nerve is always injured or deſtroyed. Its blindneſs is ſometimes tranſient, but moſt commonly permanent. It ſhould be added, that thoſe diſorders of the head, in which the electric fluid *is ſaid* to give relief, are chiefly of the nervous kind: ſuch are the gutta ſerena, locked-jaw, palſey, &c.

If the diaphragm be brought into the circuit of a ſurface, equal to two feet, fully charged, the lungs make a ſudden effort, which is followed by a loud ſhout. If the charge, ſent through the diaphragm, be ſmall, it never fails to excite a violent fit of laughter. The caſes are without number which juſtify this obſervation. I have made the trial on thoſe whoſe calmneſs and ſolemnity are never diſturbed by the ludicrous occurrences of life; but I have ſcarcely known the inſtance in which they could reſiſt the comic powers of electricity. The effects of the ſhock on the diaphragm are not tranſitory; if it has been violently ſhaken, the perſon, on whom the experiment is made, ſobs and ſighs, and burſts into tears; frequently a fainting fit will follow, attended by ſtrong hyſterical ſymptoms.

If the charge be ſent through the abdomen, ſo as to take the bladder and the rectum in its paſſage, it produces in many cafes an inſtantaneous diſcharge of the urine and fœces. Some electricians have aſſerted, that the moſt obſtinate coſtiveneſs may be thus conquered. I have myſelf been witneſs to its removal of female obſtructions of long duration. Indeed, the effects of the charge on women of irritable habits, are ſo very ſudden, that moſt diſtreſſing cataſtrophes, even in large companies, have attended the improper direction of it.

It was the conceit of an impudent empyric, that electricity could reſtore the vigour which nature takes away from the intemperate, and could make the bed of profligate effeminacy prolific. To the ſtimulants of a Mahometan paradiſe, he propoſed the addition of an atmoſphere charged with electric particles, and this propoſal was privately defended by many perſons of information, as perfectly philoſophical. I ſhould for my own part as ſoon expect a flame from exhauſted embers, as the ſpirit of a man from a ſyſtem mangled and unnerved by vice. A partial bliſter will ſtimulate a carcaſe, from which the laſt breath is vaniſhing; but its effects are no evidences of remaining ſtrength or returning life.

When the electric fluid is conveyed through the ſpinal marrow, it produces, according to the height of charge employed in the experiment, different degrees of incapacity in the lower extremities. If a perſon be ſtanding at the time he receives it, he is ſometimes brought upon his knees, and frequently falls proſtrate on the floor.

The paſſage of the ſhock through the ſpinal marrow has been recommended as efficacious to thoſe patients who, by long ſtanding in damp ſituations, or by working in atmoſpheres loaded with metallic calces, have loſt the uſe of their limbs. I have tried this remedy in three caſes.—In one of them the patient was deciſively cured;

In the ſecond, the patient became better; but her eagerneſs to get well, led her to try other ſtrong ſtimulants at the time, and by this means ſhe rendered the experiment altogether indeciſive. In the third inſtance, I obſerved what ſeems to me to be the general concomitant of applying the charge unſeaſonably. The patient's terrors grew with every repetition of the ſhock, and its feelings became ſo irritable, as at length to render a ſubmiſſion to the ſmalleſt charge a taſk of reſolution, attended with trembling and horror.

To prevent the injurious conſequences of this terror on the nerves, ſome of the faculty have recommended, that ſuch ſmall portions only of the fluid ſhould be applied, as will paſs from a point to the affected part. Nay, metallic ponts have been conſidered as too potent, and wooden ones are ſubſtituted in all the medico electrical apparatus that I have ſeen. This, I would obſerve, is a ſure method of rendering the application perfectly harmleſs; for I think it is very clear, from many of the experiments which I have ſhewn to you, that the fluid, in ſuch an application, never enters the ſkin, but always paſſes along the ſurface to the body by which it is conducted to the ground. If the patient be placed on an inſulated ſtool, and ſparks be taken from the diſordered part, in this caſe again, the ſkin alone is affected; for the ſpark is nothing more than a diſcharge of the atmoſphere ſurrounding the body, by means of the conducting particles which lie on the ſurface and ſerve as a coating.

From the preceding view of the power poſſeſſed by the electric fluid, to change, to injure, and to deſtroy the human ſyſtem, we are naturally urged to thoſe endeavours which may aſcertain, 1ſt, The diſtance at which we may be reached by the force of ſuch accumulations of the fluid as take place in nature. 2dly, The protection that may be derived from the known principles of the ſcience.

1ſt, The greateſt danger of a

thunder-ſtorm lies between the two neareſt extremities of the correſpondent parts of the charged atmoſphere, or in that interval of unelectrified air which is always found to ſeparate the poſitive from the negative portion of the loaded cloud: but on either ſide of this interval, the further you get into the poſitive or the negative, the more does the power of injuring diminiſh.

The idea which I now wiſh to impreſs, will be illuſtrated by the circumſtances of a fact, to which I have already referred.

Take a Leyden phial, five inches in diameter, and thirteen or fourteen inches in height. On the inſide, let the coating riſe till its upper edge be two inches and a half from the rim of the veſſel. On the outſide, let the coating riſe no higher than one inch from the bottom. When the phial is thus coated, let it be charged, and a ſpark will paſs from the tin foil on the outſide to that on the inſide; but its form will reſemble that of a tree, whoſe trunk will increaſe in magnitude and brilliancy, and conſequently in power, as it approaches the edge, owing to ramifications which it collects from all parts of the glaſs. Within two inches of the edge it becomes one body or ſtream, and along that interval its greateſt force acts.

When two clouds, or the two

correſpondent parts of a cloud, have their equilibrium reſtored by a diſcharge, the appearances are exactly ſimilar to thoſe of the preceding experiment. Each extremity of the flaſh is formed by a multitude of little ſtreams, which gather into one body, whoſe power is undivided in that interval only which ſeparates the poſitive from the negative.

In this country theſe appearances are frequently ſeen; but they are moſt commonly hidden by intervening clouds. While I was paſſing over mount Jura, one night during a thunder-ſtorm, the flaſhes ſucceeded each other ſo rapidly, that about thirty ſtruck within each minute; but owing to the height of my ſituation at that time, not one of them appeared otherwiſe than partially or generally, according to the deſcription I have juſt given. Sometimes a lower cloud would hide one of the two charged parts, and in this caſe the lightning aſſumed the form of a tree, whoſe trunk and branches only appeared. Sometimes the trunk was hidden, and then the ramifications on each ſide were alone viſible. Frequently intervening clouds would hide all but the trunk, and the lightning then appeared as it commonly does to a ſpectator in a low ſituation.

It muſt be obvious, from the preceding ſtatement of circumſtances, that the greateſt devaſtation of lightning muſt take place in that interval through which the whole body of the fluid paſſes, and that as you penetrate further and further into the cloud, the ſtream that is formed becomes leſs and leſs, like a river which diminiſhes by entwiſting itſelf as you approach its fountain. Hence, to us placed on the ground, no danger can ever occur, till the clouds are ſo low, that the ſtriking diſtance through air, or the aerial interval between the charged parts, reſiſts the paſſage more powerfully than the body of earth, and any additional portion of atmoſphere which may in the direction of the earth form the ſtriking interval.

If the charged cloud lies in contact with the ground, its paſſage to the earth will be that of ſeveral ſtreams, and the danger will be great, in proportion to the magnitude of that ſeparate ſtream which paſſes through any given part of the earth, and ſeveral diſtinct ſituations may be thus unequally endangered at the ſame time. Hence it happens, that the ſame ſtroke will frequently injure ſeveral diſtinct buildings, which are very near to each other, and that different degrees of injury are always obſerved in the different tracks.

The ſtriking diſtance, or the length of the interval of greateſt danger, will vary with the *height* of the charge, and not with the dimenſions of the charged body. This is clear from a multitude of facts already illuſtrated and applied. We may hence ſafely conclude, that the longer any charged cloud is in the vicinity of the wet ground, the more will the length, and conſequently the danger, of its ſtriking diſtance be diminiſhed, provided the points and prominences, which are active on the ground, diſcharge the fluid more abundantly than it is accumulated by the producing cauſe.

From what I have already ſaid, it is clear that all the parts of the circuit, through which a thunder-cloud may diſcharge its contents, are not equally dangerous, and that the maximum of danger is confined within much narrower limits than thoſe of the interval, within which it may be felt in one inferior degree or another. You muſt, however, perceive, that as the cloud enlarges, the number of additions increaſes, by which the great body of the flaſh is formed, and that the length of the moſt dangerous interval will always increaſe with and bear a certain proportion to the diameter of the cloud. In our attempts to eſtimate this diameter, we may follow two methods, which have been recommended; but I cannot ſay that either of theſe methods has any great pretenſions to accuracy.

1ſt, If you meaſure the ſpace on which the thunder ſhower falls, it is ſaid that you meaſure what is commenſurate with the dimenſions of the thunder *cloud*. In a mountainous country, this meaſurement is very poſſible; for the body of the ſhower may be ſeen at a ſmall diſtance, well deſcribed upon the elevated grounds whoſe parts it ſeparates from the eye. Its diameter, therefore, may be correctly eſtimated from the diſtance of thoſe well-known objects by which it is bounded. Thoſe thunder ſhowers, which I have obſerved, have varied in their diameter from five hundred yards to two miles. It is, however, to be obſerved, that the partial vacuum, produced by the collapſe attending the removal of the electric fluid, may extend its influence to a great diſtance, and cauſe the fall of rain by rarefying the atmoſphere far beyond the bounds of the charged cloud.

2dly, The velocity of a cloud may be known by meaſuring its height, and the time which any fixed appearance in it takes to deſcribe a certain angle. This may be done in a very ſmall portion of time, and when it is done, you are next to watch the moment at which it begins to affect your elevated conductor, and with equal accuracy you are to mark the evaneſcence of its ſigns. The knowledge of theſe circumſtances, united with that of the cloud's velocity, will correctly determine its dimenſions.

From a diary in my poſſeſſion, made by Mr. Brook, it does not appear that the ſame electricity ever laſted more than fifteen minutes. When the ſymptoms of approaching thunder were deciſive, the oppoſite electricity generally laſted as long, and the interval of time between the two electrics ſeldom exceeded one-tenth of the whole.

If we allow that the cloud in this caſe moved at the rate of eight miles in an hour, its diameter muſt have been four miles. However, in many inſtances, all the above-mentioned charges of electricity took place in two minutes. This happened ſeveral times ſucceſſively, and each ſeries of changes terminated by a flaſh of lightning. In all inſtances of this kind, to make the diameter of the cloud half a mile, we muſt ſuppoſe that it moves at the rate of thirty miles an hour; and in ſuch a caſe, one-tenth of the whole, or the interval of greateſt danger, would not exceed one hundred and eighty yards. But on the ſuppoſition that the ſize of the cloud were ſuch as to ſtrike over the diſtance of two miles, many are the circumſtances which, on its deſcent towards the ground, will encroach upon its offenſive powers, change its direction, or decreaſe, and perhaps altogether annihilate its violence.

1ſt, Innumerable points and prominences riſe from the whole ſurface of earth over which it hangs. Theſe act as ſo many channels, through which its contents will find a rapid evacuation. In the power of carrying off the fluid gradually, I have been able to diſcover but little difference between partial and metallic conductors. It ſhould be added, that the torrent through an elevated wire is ſuch, when the cloud approaches it, as would diſcharge a battery, whoſe ſurface equalled four or five acres, in twenty or thirty ſeconds. When, therefore, millions of other conductors are acting with equal effect at the ſame moment, that muſt be an immenſe cloud indeed, whoſe ſtriking diſtance in ſuch circumſtances is not much leſſened, or whoſe ſtriking powers are not altogether exhauſted.

2dly, Metals alone conduct the fluid better than *charged ſurfaces.* If a plate of glaſs, coated on one ſide with tin foil, be charged and placed in a circuit, ſo that the contents of a jar may paſs over the other ſide uncoated, the *luminous ſtriking diſtance* will be quadruple what it is in air. Such a combination of charges as that which I have now deſcribed, muſt frequently occur in the upper regions of the atmoſphere; for the charged clouds muſt lie in ſtrata above each other; and in the varieties of their motions, produced by their mutual attractions and by the innumerable cauſes which affect their different currents, they muſt be perpetually ſerving as diſcharging-rods to one another. We conſequently find that nine hundred and ninety-nine flaſhes out of a thouſand ſtrike from cloud to cloud through the intervening air.

There is, however, a queſtion of ſafety that has occurred to ſeveral eminent electricians: Are we not endangered, even from diſcharges in the upper regions of the atmoſphere? Lord Stanhope, and others who adopt his theory, anſwer in the affirmative. They aſſert, that a man may be killed at the diſtance of three miles, or perhaps at a diſtance '*prodigiouſly* greater from the flaſh.' I am not alarmed by this formidable repreſentation of dangers, for I have yet ſeen no evidence, either from fact or acknowledged principles, in favour of a theory ſo fully charged with terrors as that of the *returning ſtroke.*

I ſhall now give you an explanation of this theory, together with the arguments or facts on which it is founded. I ſhall then briefly explain to you the mode in which it is applied to the phænomena of nature, and I ſhall blend with the whole a ſtatement of my own objections.

It is ſaid, that when a charged cloud approaches another body, by the repellent power of its charge, it drives a part of its natural ſhare from the other body, which, by this means, becomes negative in proportion to the degree of ſuperabundance in the cloud. When, therefore, the cloud is diſcharged, what was driven by its repulſion out of the neighbouring body, returns into it with a force proportional to its quantity, which force they ſay is frequently adequate to the deſtruction of the body.

In like manner, when two clouds approach the earth, they repel from a given portion of it, a certain quantity of its natural ſhare to a more remote ſituation, and conſequently, when one of theſe clouds diſcharges its contents into the other, the repelled portion in the earth returns to its firſt ſituation, and its return is attended with havock and devaſtation.

It is but juſt to Lord Stanhope,

to obſerve, that *He* does not uſe the word Repulſion. The following is an abridgment of his own deſcription of his theory.

FIG. L.

PC repreſents a conductor charged poſitively. AB, a ſmaller conductor not charged, is placed ſo that its end, A, may '*juſt enter*' the atmoſphere of PC. In this caſe, he ſays, " That the ſuperabundance of PC, *owing to the nature of an electrical equilibrium*, will cauſe ſome of the natural ſhare of AB to paſs from A to B, where it is ſtopped and accumulated. By this change, A is left in a different or negative ſtate, and B, by the addition it has received, becomes poſitive.

But when the ſuperabundance is taken away from P, the poſitive at B ruſhes back to A, and this reſtoration of the ſtroke to its natural place, is called the *Returning Stroke.*

Let PC be negatively charged, and A placed as before, juſt within its atmoſphere; *according to the nature of an electrical equilibrium,* ſome of its natural ſhare will ruſh from B to A; but by the reſiſtance it will be ſtopped, and form a collection of the fluid. B, of courſe, is left in a negative ſtate. When PC is diſcharged, its returning ſtroke is then from A to B.

The cauſation of effects, which in this theory is expreſſed by the words,—" According to the nature of an electrical equilibrium," is in another place expreſſed by ſaying, " That the atmoſphere of PC preſſes againſt A, and *tends to enter*." This language is not altogether intelligible; but if Lord Stanhope's be the general idea of an electrical equilibrium, I do not think it can be reconciled to the powers aſcribed to it in his theory; for if there be any one principle in the ſcience that admits of no doubt, it is the following:

When a ſurface or a body is charged either poſitive or negative, it will neither give nor take, but in conſequence of an equal addition to, or ſubtraction from, the correſpondent negative or poſitive. If a Leyden phial be charged and inſulated, in this ſituation its poſitive ſurface, brought near a conductor negatively charged, will not part with any of its contents. In like manner, if two portions of air be charged, their poſitive and negative are perfectly immovable and inactive, till ſome connexion be formed between them; into either, properly inſulated, the fineſt electroſcope may be introduced without ſhewing the fainteſt ſigns of electricity. If this be the caſe, by what power is the atmoſphere of PC, in Lord Stanhope's experiment, to drive away a part of its fluid from AB? for AB is never once ſuppoſed to be in the negative that correſponds to the poſitive of PC. This, however, is really the truth, but it takes away the neceſſity of the theory. AB, in the experiment, when it gives contrary ſigns to PC, is always at a conſiderable diſtance; when the conductor, PC, was ſix feet long, AB would not give oppoſite ſigns till it was ſeparated by an interval of three feet, or till it came to that part of the air which correſponded to the atmoſphere of PC.

Again, if Lord Stanhope be right, why, when AB is brought nearer to PC, do not the effects which he deſcribes take place? Why does not the atmoſphere of PC puſh away part of AB at one diſtance as well as another? Indeed, if his reaſoning be juſt, it might be expected that the power which operated on AB would increaſe as it approached the conductor; but the very reverſe takes place.

Again, AB, a metallic body, is ſaid to be in two different ſtates at the ſame time. What ſingle electrical fact is there to warrant this aſſertion? What is there intelligible in electricity, if we admit that perfect conductors can have their equilibrium diſturbed, or that two different parts of them can be at the ſame time in two different ſtates, when there is no kind of inſulation to ſeparate the poſitive from the negative, but, on the contrary, ſuch a communication as in every other inſtance immediately reſtores an equilibrium?

You will obſerve, that Lord Stanhope's experiment is made on metal ſurrounded with air; and why, contrary to every known principle, ſhould that effect be conſidered as belonging to the metal, when it is agreeable to every known principle, that the air alone ſhould be affected? Had AB been confined in a glaſs tube, having its end, A, only expoſed, if Lord S. be right, there would have been no alteration in the effects he deſcribes; for PC would ſtill poſſeſs the ſame power to puſh the particles at A to the end, B. Indeed, the additional reſiſtance at B would rather aſſiſt the accumulation that is ſuppoſed to take place there. However, the reſult of the experiment would have been totally different from what the theory required.

Again, Lord Stanhope's theory is not wanted to account for the appearances of that experiment on which it is founded. The various ſigns given by pith balls, placed on AB, are all the conſequences of its being connected at the ſame time with two different atmoſpheres. I could place pith balls between the poſitive and negative ſurfaces of a Leyden phial, which would diſcover ſimilar varieties in their ſigns, and which could be explained by ſimilar principles.

If the preceding theory be erroneous, the application of it muſt be nugatory; but if it were true, ſtill the application might be falſe. This is the caſe here; I ſee no analogy between the experiments and the natural phœnomena they are deſigned to elucidate. If PC be conſidered as a cloud, we cannot admit what the theory requires, that AB ſhould be the repreſentative of the earth, or that what happens to an inſulated, ſhould be regarded as equally true of an uninſulated body.--- The difference here between what is required and what is true, is as great as can ſeparate one caſe from another. Again, when PC is diſcharged, Lord S. ſays, that the negative in one part of AB will be ſupplied by the poſitive ruſhing from the other, and that the reſtoration of the equilibrium would be attended with violence enough to deſtroy men, houſes, and other objects of great magnitude.

Let us allow that the force required by the theory is rendered active in the manner which I have juſt deſcribed, what reaſon have we for believing that it would be active to the *degree ſuppoſed?* Lord Stanhope has eſtimated, that what is ſeparated from our natural ſhare *without injuring us*, and what may be *abſent for hours without being felt*, is ſo great in quantity as to deſtroy us by its motion in returning. But what are the grounds of this eſtimate? As yet it has been juſtified by no appeal, either to fact or experiment; and the perſon who could ſay, that the greateſt poſſible loſs from our natural ſhare is little or nothing, would certainly ſtand upon equal, I think rather better, grounds than thoſe who would make it adequate to the fuſion of metals and the deſtruction of life. I would add, that when the power of the returning ſtroke is magnified as it is in the theory, the rationale of this bold eſtimate is not only neglected, but it is neglected where it might have been made without much trouble.

If the returning ſtroke of a thunder-cloud will deſtroy large edifices, ſurely artificial electricity could produce a ſimilar ſtroke, which would deſtroy a bird, or a mouſe, or act on ſome ſcale analogous to that which it is ſaid to reſemble. If I ſay the returning ſtroke in nature will melt the irons of a waggon-wheel, ſurely, with the grand machines which we are now able to conſtruct, ſuch a returning ſtroke might be cauſed as would melt a capillary thread of metal. But nothing of the kind has ever been done or attempted by thoſe who ſupport the theory, and I am bold enough to propheſy, from the details of my own experience, that nothing of the kind ever will be done.

Give me leave to add, that what I have before aſſerted concerning the theory itſelf, is equally true of its application. The *whole is unneceſſary.* Every natural appearance may be eaſily accounted for without it. If a cloud be very large in its dimenſions, or if its contents occupy a body of air whoſe diameter is equal to the length of two or three miles, during its diſcharge the ſuperabundance will be gathering into one maſs by ruſhing through different channels, and whatever partial conductors are near, will ſerve to connect the different parts of the cloud, ſo that at the time of the ſtroke, and at a conſiderable diſtance from its greateſt violence, a number of different objects may be ſubject to its power, for they are evidently placed within its circuit.

I recur to the ſubject from which we deviated in out purſuit of Lord Stanhope's theory. We have already endeavoured to inveſtigate the chief peculiarities attending the diſcharge of a thunder-cloud; to ſpecify the varieties of danger which attend it, and to determine the circumſtances from which their extent may be eſtimated.

I ſhall now proceed to deſcribe thoſe ſigns of approaching danger which may be ſelected as the moſt unequivocal and deciſive. They are the following:

1ſt, *A rapid approach of the charged clouds.*—The longer any given portion of charged air is over the earth, the more, as I have already ſhewn, is it affected by points and prominences; but when a cloud ſeems to be no ſooner formed, than it is over our heads, we are then expoſed to its undiminiſhed fury. The ſame may be ſaid of a cloud, which, while it is near us, grows darker and darker; for, in this caſe, we may be certain that the collection is not materially leſſened by any exhauſtion, and that the charge muſt ſoon accumulate to its ſtriking height.

2dly, *The perpendicular direction of the flaſhes.*—This is a certain evidence that the charged clouds are at that height from which they can ſtrike into the ground. Some electricians have conſidered the appearances of two flaſhes at the ſame moment as an evidence that the earth is acting as a diſcharging-rod. But though this may often happen, as often the two extremities of the flaſh, when paſſing behind a cloud which partly hides it, will ſhew the ſame appearance. The ſign, therefore, is not ſufficiently appropriate, and therefore cannot be conſidered as expreſſing more than a certain degree of probability.

3dly, An inſulated conductor, whoſe language, to thoſe who are acquainted with it, is ſeldom equivocal; a torrent of ſparks flows thro' its interruption when danger is near. If that torrent, after continuing for ſome time, ſhould ſuddenly ſtop, and again, after an entire abſence of all ſigns, ſhould rapidly return with an electricity oppoſite to what it ſhewed at firſt, it is certain, that during the ceſſation of ſparks, the conductor paſſed the limits of the greateſt danger. The torrent of fluid, whoſe ſtrength and rapidity are peculiar to this caſe, is attended with a loud and ſingular ſucceſſion of ſounds, Mr. Brook very accurately compares it to the noiſe made by a church or turret clock, immediately after it has ceaſed ſtriking.

Let us next ſuppoſe that we are juſtified, by the preceding ſigns, in apprehending that our ſituation is within the ſtriking diſtance, it is an object of importance, in ſuch circumſtances, to know how the threatened injury may be avoided.

If the lightning ſhould enter your dwelling, you are certain it will paſs from one partial conductor to another, and that this ſeries of partial conductors is always the appendage of walls and partitions: you will therefore place yourſelves in the middle of a room, becauſe the air will then ſeparate you from the ceiling and the ſides of the room, and will form ſuch an interruption in the circuit as muſt neceſſarily determine the preference of the ſtroke through the connected parts of the building.

In the open air, it is the ſafeſt method to keep at a diſtance from all high and pointed objects; for if you ſtand near them during the ſtorm, you may become part of their circuit, ſimilarly to what happens when you are near a wall or a partition. Beſides, if the object be ſtricken, you may be deſtroyed by the circumſtances of its deſtruction. Many are the inſtances in which travellers, who have fled under high trees for ſhelter, have been either killed by the lightning, or daſhed to pieces by ſplinters of wood.

In the open air, you ſhould avoid every approach to rivers and brooks, and more eſpecially to thoſe freſh ſtreams of water which have been formed by the thunder-ſhower; for they are all excellent diſcharging-rods, and when it is joined to them, the height of a human being furniſhes a prominence very likely to determine the courſe and to lengthen the ſtriking diſtance of the lightning.

In the open air, any covered carriage is a protection, provided you can ſit in it at the diſtance of ſome inches from the ſides. Indeed, vehicles of this kind might be made the ſafeſt of all poſſible ſituations, by ſurrounding them at the top and the bottom with thin metallic fillets, connected with each other by a ſtrip of the ſame ſubſtance.

It is admitted by all electricians, that buildings and ſhips may, by certain means, be completely protected from the violence of lightning; but the reputation of every method, hitherto preſcribed, has been ſhaken by accidents of injury within the ſuppoſed ſphere of their activity. Many are the inſtances in which houſes have been damaged when armed with a ſingle pointed conductor; and we have ſeen much labour beſtowed on the inveſtigation of one frightful caſe, in which a building, ſecured by ſix pointed conductors, was ſet on fire by a *ſingle flaſh*.

It appears to me, that the beſt means of protection can be known only in conſequence of a thorough ſearch into the various combinations of perilous circumſtances which can occur. When theſe are clearly underſtood, we have every neceſſary direction for the ſituation of our rods; in other words, our conductors may be ſo placed, that the lightning cannot poſſibly enter into any circuit of which they are not a part.

FIG. LI.

Let B repreſent the poſition of the charged cloud; A, the part that is oppoſitely charged, or that is connected with it; FG, a pointed wire. In this caſe, the electric fluid muſt paſs either through the ſeries of partial conductors, a, b, c, &c. or through the body of earth, AF.

Now when, on the one hand,

we conſider the dryneſs of that ſoil which is generally ſelected for the foundation of buildings, the probability there is that nothing but the ſoil, thus dry, may ſeparate A from the wire FG, and the certainty that if water ſhould connect A and FG, its reſiſtance is very conſiderable; when, on the other hand, we take into conſideration, the nails, bolts, iron bars, ſtrips of lead, bell-wires, and metallic utenſils that are ſcattered through all buildings, we ſhall, I think, perceive the much greater probability there is of the lightning's paſſing through a, b, c, d, &c. to the cloud, than of its paſſage through the ground.

2dly, Let us erect another wire, HI, and ſtill the danger is almoſt as great; for now the poſſible circuits of the lightning are four, and of theſe, that leading through the houſe appears to be the eaſieſt. If HI convey it harmleſs, then it muſt paſs through the body of air, FG, or over the roof of the houſe. We well know, from paſt experiments, that the inſulating power of the air makes the reſiſtance in the direction IG very conſiderable, and even on the ſuppoſition that i were wet, the reſiſtance over the roof of the houſe is not much leſs conſiderable. If the houſe were covered or coped with lead, the probability of a ſtroke would then be diminiſhed, but not taken away; for, ſuppoſe the eaſieſt circuit ſhould lie in the direction KM, then, rather than paſs through the body of earth HK, or FK, it might find an eaſier paſſage through the houſe than either of the conductors. This would not be the caſe, if a ſtrip of lead, or metallic ſubſtance of any kind, extended from K to H, and K to F. I hence thought, at one time, that a houſe would be perfectly ſafe, if a ſtrip of lead were carried around the top and all the bottom of the building, and then connected by two or three metallic ſtrips extending from the one to the other.

In conſequence, however, of obſerving the peculiar effects of lightning on certain trees which had been deſtroyed by it, a caſe occurred to me, againſt which the preceding method is no protection.

Though a great part of the charge, in the inſtance I allude to, had taken its courſe under the bark, ſo as to peel off the whole of it completely, ſtill a vaſt quantity had penetrated through the center of the trees, with ſuch force as to quarter them. The roots of a tree, ſpread in a great variety of directions, and into as great a variety of depths; conſequently, the poſſible circuits of the fluid, when it ſtrikes through a tree, being as numerous as its roots, muſt reſemble them in their varieties. Should it take that which turns up immediately into the trunk, the ſtroke will then paſs through the heart of the wood, and neceſſarily produce the effects I have deſcribed.

Let us ſuppoſe that a houſe were erected over a ſtratum of moiſture, or any other conducting ſubſtance, which dipped conſiderably, at a little diſtance from the houſe, and then ſuddenly roſe juſt below it; in that caſe, if the ſtratum became the circuit of a charge, the ſtroke would riſe immediately in the center or body of the houſe, and in all directions would force its way, with devaſtation, towards the conductors on the outſide.

All *poſſible* danger may be avoided, if, when the houſe is built, the foundation of each partition-wall be laid on a ſtrip of lead, or if the lead be faſtened to the ſides of them. The ſeveral ſtrips ſhould be connected, and the dimenſions ſhould not be leſs than one-fourth of an inch thick, and two inches wide. A perpendicular ſtrip, on each ſide of the houſe, ſhould riſe from this bed of metallic conductors to the ſurface of the ground: there a ſtrip ſhould be continued around all the houſe, and carefully connected with water-pipes, &c. The ſtrips on the ſides of the houſe ſhould then be continued to the roof, where the method of guarding the bottom ſhould be imitated. The top ſhould be ſurrounded by a ſtrip, whoſe connexion ſhould ſpread over every edge and prominence, and hence ſhould continue to the ſummit of each ſeparate chimney.

I would particularly ſpecify the protection of the chimnies; for I have been witneſs to a caſe in which a houſe was guarded, in moſt reſpects, according to the method I have juſt deſcribed. The chimnies alone were unprotected; the lightning conſequently ſtruck into one of them, where its rage terminated; but the tumbling of the chimney into the roof was attended with ſerious conſequences. You ſhould remember, that by guarding your houſe, you make it of all objects, that which is the moſt likely to become the circuit of a cloud; you conſequently ſhould be very careful that no interruption divides your conductors, for there havock will probably take place.

The expence of a conductor, erected according to the plan I have deſcribed, may be objected to as very great. I would anſwer, that the principal portion of this expence may be avoided, if we make a proper uſe of the leaden pipes and copings which belong to moſt houſes, and if we employ the common plumber or the blackſmith to do the buſineſs; for if the directions I have given you, be remembered, no other ſkill is requiſite, than that of faſtening the ſtrips of lead, ſo that they may be ſecure and at the ſame time connected with each other. This buſineſs will certainly be magnified into a vaſt enterprize, if ſuperintended by a profeſſional man. He will take care to uſe metal enough, and to ſwell the importance of his employment, by making it as burthenſome as poſſible to his employer.

I have ſeen inſtances in which practitioners of this kind have raiſed bars of iron to the height of forty feet, cloſe to a large leaden pipe of equal extent.

There can be little difficulty in protecting ſhips. One ſtrip of metal ſhould ſurround the deck; another ſhould be faſtened to the bottom or the ſide of the keel: theſe ſtrips ſhould be connected with others which embrace the ſhip in different parts. If the veſſel be copper-bottomed, nothing more is neceſſary than to connect the metal which ſurrounds the deck with the copper; but, in both caſes, a ſeparate ſtrip ſhould paſs from the reſt of the ſtrips to each maſt; no injury can then poſſibly happen below deck. This is a circumſtance of conſiderable importance; for the conductors, which are uſually deſigned for the maſts, are moveable, and injury has often been the conſequence of neglecting to place them in their proper ſituation.

The protection of the maſts muſt be managed by extending a metallic body along the ſtays to as great a height as poſſible. Chains are frequently employed for this purpoſe; but ſtrips of lead are cheaper; they are not ſeparated by any interruptions; they are not ſo liable to injury from the weather and ſalt-water as iron is, and, I ſhould think, might be faſtened without annoying any neceſſary movement.

THE ORIGIN OR CAUSES OF NATURAL ELECTRICITY.

Having now proved that all the appearances of thunder proceed from the agency of the electric fluid, or its paſſage from one part of our atmoſphere to another; having likewiſe deſcribed and endeavoured to illuſtrate the ſeveral peculiarities of *ſound*, *light*, *direction*, *and form*, which belong to theſe appearances; and, finally, having endeavoured to eſtimate the dangers attending them, to mark their ſigns and to point out the beſt protection againſt their violence,—I ſhall now proceed to inveſtigate their *cauſes.*

From time immemorial this ſubject has ſtimulated the human mind to be active and buſy in conjecturing, and to us, enlightened as we are by diſcoveries of a very recent date, the imaginations of theoriſts, who ſpeculated near two thouſand years ago, appear not leſs rational than thoſe of many eminent moderns, who have ſcarcely cloſed their eyes on the brightening day of philoſophy. In ſome particulars, Lucretius approached as near the truth as Dr. Franklin. The former aſcribed thunder to the contending percuſſions of one cloud againſt another, and the latter, to the friction of the ſaline againſt the aqueous particles of the ocean.

By Mr. Bennet's electroſcope, we are taught that whenever a ſolution or precipitation takes place, or whenever any two bodies, having a mutual attraction to each other, are united or ſeparated, a change, attended by electrical ſigns, is immediately produced. This is particularly the caſe in air, and the change is never ſo conſiderable as when its component quantity of moiſture is either increaſed or depoſited. In our endeavours, then, to explain the production of natural electricity, we have nothing more to do, than to diſcover the various circumſtances of the atmoſphere, nothing more to do than to diſcover the various circumſtances in which moiſture is abſorbed or precipitated. When theſe are known, it can be no difficult buſineſs to find out the ſeveral partial and leſs powerful cauſes which may either increaſe or diminiſh the effects of the general and moſt powerful cauſe. It is, however, previouſly neceſſary to remind you of the proof furniſhed by numerous experiments, that when any portion of the atmoſphere is in a ſtate to take up an additional quantity of moiſture, it is in a ſtate at the ſame time to take up more electric fluid; and, vice verſa, when it is parting with its water, it is at the ſame time parting with its electric fluid. But in theſe caſes neither the ſuperabundance nor the deficiency can produce a charge, unleſs there be ſome other part of the air contemporaneouſly in an oppoſite ſtate, or in a diſpoſition either to receive or give. It is, however, ſcarcely poſſible that this ſhould not always happen; for our atmoſphere is, throughout its vaſt dimenſions, each moment agitated by millions of co-inſtantaneous changes, and for our purpoſe it is of no conſequence *where* the required change takes place. Were it in New Holland, or at the Antipodes, a connexion would be inſtantly formed between the remote but oppoſite ſituations, by the conducting power of the earth.

It is a neceſſary concluſion, from what I have juſt ſaid, that if the abſorption of moiſture by the air, or the copious evaporation of it from the earth, be attended with a new accumulation of the fluid; then *wherė* this cauſe operates moſt powerfully, *there* its correſpondent effect will be moſt ſenſible. We conſequently find, that the moſt tremendous electrical phœnomena belong to the countries within the Tropics, or to that portion of our atmoſphere which is loaded with moiſture by the moſt powerful influence of the ſun's rays. In like manner, within the limits of our own and other ſimilar climates, electrical phœnomena are greateſt, both in force and frequency, during the hotteſt months of the year, or during the ſeaſon in which our atmoſphere is moſt copiouſly and rapidly charged, by abſorbing the humidity of the ground.

In the neighbourhood of Ætna and Veſuvius, during the period of their volcanic fury, ſurfaces, covering the dimenſions of ſeveral ſquare leagues, are ſometimes ſcorched with red-hot lava, and every atom of their moiſture is rapidly diſſipated. At the ſame time the ſurrounding air is heated to a vaſt extent, and in this ſtate ſwallows up an immenſe quantity of aqueous vapour; but contemporaneouſly with the operation of theſe powers, according to the reports of all natural hiſtorians, an immenſe quantity of the electric fluid is accumulated and diſcharged.

Again, a dry wind, paſſing over a moſt ſoil, is another modification of the cauſe we are applying: it produces a copious and rapid ſolution of aqueous particles, and its conſequent alteration of attractive force. Let us, for inſtance, ſuppoſe a wind, which had paſſed over the deſerts of Arabia, or that had been well roaſted in its paſſage over a large extent of burning ſands, to come in contact with a ſimilar extent of marſhy ſoil, or of any ſurface well drenched with water, a moſt abundant evaporation would neceſſarily take place, and with it an immenſe accumulation of the electric fluid. But ſubſequently, in caſe any power operated, which would take away the aqueous particles thus diſſolved, and of courſe altered the degree of attractive force by which the collected electric fluid is ſuſpended, we ſhould find that the moſt dreadful thunderſtorms would take place. This is really the caſe; for there is ſcarcely a region in the vaſt circle ſurrounding the immeaſurable ſands of Africa, which is not remarkable for ſtorms and tempeſts.

On the ſide of Abyſſinia, when the warm winds that have paſſed over the neighbouring deſerts are condenſed on its mountains, thoſe deluges are collected which form the inundations of the Nile.

On the coaſt of Guinea, the harmattan, which is a current of air ſo dry as to wither and pulverize, by a complete abſorption of all its juices, every ſubſtance that occurs in its paſſage, is no ſooner mixed with that body of air which is cooled by the ocean, than it forms the moſt terrific hurricanes of wind and lightning that are deſcribed by navigators. Along the Syrian regions, we learn from ſacred authority, that the ſtorms gather with ſuch rapidity, that a cloud which the hand might cover this inſtant, is within the interval of a few minutes charged with water adequate to the inundation of a whole country.

The thunder that attended theſe impetuous ſtorms, provoked the ſublimeſt expreſſions of their poets. Indeed, whenever their minds attempt the deſcription of celeſtial greatneſs, or the ſudden and overwhelming approach of divine power in its triumph or in its fury, they have recourſe for imagery to thoſe thunder-clouds, which they juſtly repreſented as extinguiſhing the light of the ſun, and as involving the world in a few inſtants in the darkneſs of midnight.

In this country, the cold attending our eaſterly winds muſt, in a great degree, counteract the effects of their drought; but from Mr. Read's journal, it is evident that N. E. winds are frequently attended with conſiderable accumulations of the electric fluid.

Having ſpecified the two moſt conſiderable cauſes of evaporation on the ſurface of this earth, let us now attend to the poſſible changes of the atmoſphere, when, by the operation of either or both of them, it is charged with the electric fluid. All theſe changes are but different degrees of the ſame effect or of the condenſation of moiſture, and this condenſation is in every caſe produced by an alteration of temperature, which may proceed,

1ſt, From a mixture, or even the contact of a colder with a warm air. When the ſmalleſt clouds are formed by ſuch a mixture, it is evident, by Mr. Read's journal, that an electrical charge takes place, ſo that one portion of the cloud has more, and the other leſs than its natural ſhare. Fogs, dews, and the ſlighteſt change of clear for hazy weather, are moſt commonly the conſequences of a warmer atmoſphere coming in contact with one of different temperature, but even theſe trifling degrees of condenſation are, by Mr. Read's journal, known to be always followed by ſigns of electricity.

In this country, the air changes its appearances very often twenty times in one day, owing to its inſular ſituation, which expoſes it to the perpetual influence of varying winds. But there is no degree of thick cloudineſs or perfect clearneſs, of ſcattered clouds ſucceeding embodied maſſes of clouds, of ſmall rain increaſing to heavy rain, or of heavy decreaſing into ſmall rain, that is not attended with changes in the expreſſions of the elevated conductor, which never fails to vary with all the atmoſpheric condenſations and rarefactions that take place.

It is however obvious, that in proportion to the quantity and rapidity of the condenſation, ſo muſt the effect be. When, therefore, any body of air has been a long time ſuſpended over a ſurface of ground, previouſly drenched with ſhowers, and at the ſame time expoſed to the violence of the ſun's rays, a change in the direction of the wind, or ſuch a change in the weight of the air as mixes the upper and colder with the lower regions of the air, is almoſt always attended with a thunder-ſtorm.

In tropical climates, day follows day for months together, in which the calm atmoſphere becomes loaded by one addition of moiſture to another, till it becomes at laſt the reſervoir of vaſt rivers and lakes, and of all the moiſture that is ſpread over whole continents. But when this drought has reached its criſis, the ſun paſſes the line, the wind takes a new direction, a colder air mixes with that which is thus charged with vapours, and the condenſation becomes ſo copious as to inundate all the ſubjacent countries; but the deluge is not more deſtructive than the attendant ſtorm;

for, according to the reports of ſpectators, our imaginations, confined to the proceedings of nature in this frozen region, have no images from which any ſuch compariſon can be made, as will communicate the leaſt idea of the thunder attending a tropical hurricane.

The cauſe I am now applying to the explanation of natural appearances, will furniſh us with an eaſy ſolution for one difficulty which has oppreſſed ſeveral electrical theories; I mean that rapid generation and increaſe of electricity which takes place in ſome thunderſtorms. Even in this country, I have known the ſucceſſion of flaſhes to be ſo quick, that one hundred and twenty followed each other within a minute. In Aſia, this celerity of accumulation and diſcharge was ſo great, that Homer uſes it as part of a ſimily, by which he paints the quick repetition of Agamemnon's ſighs and pantings in an hour of diſtreſs. In the Eaſt and Weſt Indies, it is a common expreſſion,—" The ſky was fired for hours by one inceſſant blaze of lightning."

It may be aſked, If each diſtinct cloud is loaded with a diſtinct charge, and if each flaſh is a ſeparate diſcharge of ſuch a cloud, what is there, in our knowledge of natural powers, that will account for an innumerable repetition of theſe accumulations and diſcharges within a very ſmall ſpace of time, more eſpecially when each of them is connected in our minds with the neceſſity of a diſtinct part of that time for its proceſs? In other words, Do we know of any cauſe that is adequate to the filling and emptying of the ſame cloud, or, more properly, of the ſame portion of air every inſtant for hours together?

On a hot ſummer's day, within five ſeconds of time, I have frequently ſeen one fine blue ſky changed into one maſs of clouds, which occupied the whole viſible hemiſphere. If the cauſe which produced ſo great an effect, were ſuppoſed to be doubled in its influence or its power of condenſation, the degree of electricity ſhewn by the elevated conductor would be thus rather more than doubled, and its ſigns would be much ſtronger than what it ſhewed when a common ſtorm is over it; we ſhould hence conclude, that the whole maſs, which might be thus formed in five ſeconds, would be loaded ſo as to have every part of it at the diſcharging height: but the maſs might conſiſt of hundreds of diſtinct clouds, all in the ſame ſtate, and conſequently adequate to the appearance of ſeveral hundreds of flaſhes within one minute.

The collapſe of aqueous particles, which would neceſſarily follow ſuch a rapid ſucceſſion of diſcharges as I have now proved to be poſſible, would produce a partial vacuum of great extent, and on all ſides the heavier air would ruſh into it, and the upper and colder regions would preſs downwards, and by their condenſing temperatures would renovate all the accumulatious and diſcharges which I have already deſcribed; a ſecond collapſe would follow a ſecond ſeries of thunder-ſtrokes, and a partial vacuum additional to the former; a freſh portion of warm air would again ruſh in from all quarters, and a freſh mixture of cold air from the upper regions. It is ſcarcely neceſſary to ſhew that this repetition of condenſations may continue for hours, or till the air, which ruſhes in laterally, becomes of ſuch a temperature, that its mixture with the colder air will not produce the condenſations adequate to the collection of that quantity of electric fluid which is neceſſary for a diſcharge.

From this explanation it is obvious that a central point muſt exiſt, at which the violence of every ſtorm begins, and from which it is ſpread in all directions. A hurricane in the Weſt Indies, though ruinous to many, is generally the diſtinguiſhing calamity of one iſland, at which alone the wind is deſcribed as blowing from every point of the compaſs; while, in every other iſland, it is repreſented as bearing down deciſively from one quarter.

2dly, The precipitation of aqueous particles, when ſuſpended by heat in air, is frequently the conſequence of the loaded atmoſphere's coming in contact with portions of the earth that are colder than itſelf. Such, particularly, are the ſummits of mountains, whoſe effect is great in proportion to the degree of their cold and the extent of their ſurface. It is however certain, that condenſations, when thus produced, are invariably attended by thunder-ſtorms.

The darkneſs, the uproar, and the ſplendour of the innumerable lightnings, which dart thro' all the entangled circuits of an abyſs of thunder-clouds, are the immutable attributes of grandeur which belong to the cordilleras;

for they dam up, as it were, an immenſe flow of air, which is almoſt ſaturated with moiſture by paſſing over thouſands of leagues of land expoſed to the fury of a tropical ſun.

In ſummer, the north-weſterly winds that paſs over France are always condenſed by the Alps, and in the night, during ſuch a ſtate of the atmoſphere, to all thoſe who live along the Soane and the upper part of the Rhone, theſe mountains are always brightened by electrical flaſhes and corruſcations.

All ridges or chains of very high grounds, eſpecially thoſe which terminate extenſive plains lying in the direction of their moſt common winds, are perpetually beclouded, and with a good conductor, fixed on their ſummits, we ſhould find that the ſigns of electricity were as conſtant as the condenſations by which they are enveloped. But in proportion to the coldneſs, ſo is the ſubſequent change of temperature which takes place on the eminences diminiſhed, and the electrical effect dependant on that change. It hence happens that there are countries in the northern parts of Europe, the gloom of whoſe miſts is never diſperſed by a thunder-ſtorm, excepting in the hotteſt ſeaſon of ſummer.

3dly, When the ſun, by directing its rays with force and abundance upon the earth for any length of time, has produced a conſiderable evaporation, the mere interruption of its influence will be attended by a diſcharge of the electric fluid; for the great ſource of change in our atmoſphere is the ready influence of its upper regions, which are cold, on its lower regions when warmed, and any cauſe which mixes theſe together, muſt bring on a ſudden condenſation of aqueous vapour. This mixture, however, takes place on the mere approach of night, as is evident from the change of temperature expreſſed by the thermometer, and the uſual fall of the dews: we conſequently find, that as night comes on, the ſigns of electricity always increaſe. When the weather is tolerably ſettled, or ſuch that no other cauſe is active than that proceeding from the change of day for night, or night for day; then, according to Mr. Read's obſervations, the ſigns of electricity gradually decreaſe from twelve o'clock at night till ſix in the morning; from this hour till nine, they gradually increaſe, when they become exceedingly weak, and continue ſo till four in the afternoon: the increaſe at this time re-commences, and is very deciſive in its appearance till about two hours after ſun-ſet, when it becomes ſtationary, and remains in this ſtate, or decreaſing, ſo as ſcarcely to be ſenſible, till the morning.

If we were to deſcribe the preceding facts in the language ſuggeſted by the relation of their appearances to their cauſe, I ſhould ſay, that when the dews are fallen, or that condenſation ends which is cauſed by the departure of the ſun, then the change which produces electrical ſigns is removed. When the morning appears, then the rays of the ſun, warming the ground, cauſe the vapours, condenſed by the night, to re-aſcend, and, by their mixture with the air, to produce new ſigns of electricity. As ſoon as this mixture is over, or about nine o'clock, that power has ceaſed, which ſeparates any portion of the electric fluid. At four o'clock, the evening cold begins, and of courſe the evening condenſation of vapours, which lets looſe whatever quantity of the electric fluid was abſorbed during the courſe of the day.

The cauſe whoſe operation I have now inveſtigated, in the production of its moſt feeble effects, may be eaſily applied to many other cauſes, in which ſimilar but greater powers are diſplayed by nature. Let us ſuppoſe, that on a wide ſurface of ground, previouſly warmed by the ſun, copious ſhowers of rain had fallen, followed by a return of the ſun's influence; in this caſe, the evaporation is neceſſarily very rapid, and the ſigns of electricity, expreſſed by an elevated conductor, are by far the ſtrongeſt that have occurred to my experience, excepting thoſe which are given by a cloud that is in the act, as it were, of diſcharging itſelf.

When a copious production of electric fluid, ſimilar to what I have now repreſented, has attended a copious evaporation continued for ſeveral ſucceſſive hours, a thunder-ſtorm, or ſome ſtriking electrical appearance, muſt come on with the approach of night; for, unleſs the barometer ſhould riſe ſuddenly, the condenſation attending the evening's cold muſt be very conſiderable, and its uſual conſequences great in proportion. The moſt violent thunder-ſtorm which I ever witneſſed in this country followed ſuch a day as I have juſt deſcribed. Indeed, I have ſcarcely known the inſtance in which a moiſt ground, operated upon by a clear ſun for ſome hours, provided the wind continued to blow from the ſouth-weſt or weſt, was not attended the following night by the appearance of falling ſtars, northern lights or flaſhes of lightning.

I have ſeveral times alluded to the connexion of breezes or winds with the phœnomena of natural electricity. Their influence muſt depend on a great variety of circumſtances; but in ſome caſes, I ſhould ſuppoſe they would diminiſh; in others, that they would put an end to electrical appearances. When they are the current which proceeds from the mixture of two winds of different temperatures, they are then (as I have already obſerved) the effects of a condenſation which may be followed by the moſt violent ſtorms. But in caſe two regions ſhould exiſt near each other, in one of which the rays of the ſun and the moiſture of the ground co-operate in producing electricity, while, in the neighbouring region, a condenſation favourable to the diſcharge or releaſe of the electric fluid is prevalent; in ſuch a caſe, a current of air would operate like a communicating rod between two oppoſite ſurfaces; it would exchange the ſituations of the charged bodies, and conſequently would cauſe the new ſituation to counteract the effects produced in that which was juſt left. The more rapid this exchange, the more ſenſible would the conſequence be: we therefore find, by Mr. Read's diary, that high winds are always unfavourable to aerial electricity.

Again, the following is a mode in which currents of air muſt operate powerfully.

It appears, that almoſt every diſtinct cloud conſiſts of a poſitive and negative portion, whoſe influence will depend on their inſulation; but when you bring a body that is charged, ſo that its oppoſite parts may have a connexion with a body *capable* of being charged, a diviſion of the charge will take place. This diviſion will be ſubdivided by a ſucceſſion of contacts ſimilar to what I have now deſcribed, and an accumulation of conſiderable magnitude in its firſt ſtate will be thus reduced to nothing in a very ſhort ſpace of time. This, however, is exactly the effect of a wind; for it carries the cloud through a body capable of participating in its contents; each ſucceſſive part of it therefore acts like a partial conductor, which gradually ſteals away ſmall portions, till at laſt the whole of the charge is diſperſed.

NORTHERN LIGHTS.

I have now directed your attention to all the peculiar appearances of aerial electricity, and to the elucidation of them I have applied the various principles which you have learnt from the preceding parts of theſe Lectures; I ſhall now proceed to other phœnomena, which, though in reality the effects of that cauſe whoſe ſole influence we have hitherto inveſtigated, are, notwithſtanding, treated by philoſophers as a diſtinct object of conſideration.

Several of the moſt celebrated enquirers into nature have given their authority to ſome of the moſt extravagant theories, in attempting to aſſign its proper cauſe to the aurora borealis. Their imaginations have kindled bonfires in the poles of the earth, and they have repreſented the northern lights as the effects of flames, to which thoſe lights have ſcarcely any ſimilarity, and from which they are diſtinguiſhed by numberleſs diverſities.

The ſalt pits of the north were at one time regarded as emitting a luminous effluvium from their entrails, copious enough to pervade the whole of our northern atmoſphere. The diſcoveries of electricians have conſigned all theſe reveries to a ſhade, whence they would never return to excite the wonder of modern philoſophers, if the authors of them had not brought forth other productions, whoſe merits have made even their miſtakes immortal.

I will now lay before you the evidence we have for conſidering the northern lights as the effect of the electric fluid. I will then deſcribe ſome of their moſt ſtriking appearances, and, as I proceed, (wherever probability juſtifies me) I will endeavour to point out the particular manner in which the electric fluid operates.

I. If thunder be cauſed by the electric fluid, there is a certain height of the atmoſphere in which the cauſe of thunder muſt neceſſarily aſſume the appearance of the northern lights. In air, rarefied to a certain degree, the paſſage of the electric fluid is attended with all the undulating corruſcations of the aurora borealis. Indeed, there is not a ſingle circumſtance in the experiment of paſſing a ſpark or charge through an exhauſted tube, which does not bear a reſemblance to ſomething obſerved in the northern lights. There is the ſame peculiarity in their motion; the ſame variety in their colour, and the ſame quick alternations of flaſhes in both; the ſtreams are alike vivid and pointed, and if the exhauſtion be properly managed, ſome parts will appear with that reddiſh tinge, which is often obſerved in the air by the vulgar, with fear and conſternation. But

II. As the rarefaction of the air increaſes, ſo does the ſtriking diſtance of a charge that paſſes thro' it. If two clouds, therefore, the one poſitive and the other negative, ſhould have no other circuit, is it not highly probable that they will diſcharge themſelves through the higher regions of the air, eſpecially if their own elevation be conſiderable, and their communication by means of the earth impracticable?

III. The changes in the atmoſphere, which produce thunder, are moſt commonly productive of the aurora borealis; for this appearance is moſt generally ſeen during the heat of ſummer, and the inhabitants of the northern countries have obſerved the aurora to be remarkably ſtrong, when a ſudden thaw has ſucceeded very cold weather.

If you admit the ſtrength of ſuch evidence as I have now produced, with the intention of proving the identity of that cauſe on which thunder depends, to that which produces the aurora borealis, our next buſineſs will be to examine whether our knowledge of electricity will help us to account for the ſeveral peculiarities attending this phœnomenon.

1ſt, Why does it moſt commonly appear in the northern parts of the hemiſphere? This queſtion is, I think, eaſily anſwered by recurring to what we have already deſcribed and explained. As the ſun declines towards the horizon, that condenſation takes place, which we have ſeveral times proved to be one of the moſt abundant ſources of electrical appearances; but if this condenſation prevails in the upper regions of the atmoſphere, the fluid that is let looſe will certainly take its courſe through the moſt rarefied portion of the air, to that which is negatively electrified. —The condenſation, however, to which I call your attention, muſt follow the courſe of the ſun, and as it forms its continued track around the earth, the condenſation, which takes place ſucceſſively in the regions the ſun has left, will be ever ſupplying the increaſed attractive force of the region it warms. Thus, when the ſun is in the meridian of any place, it is moſt powerful, and if the evaporation be rapid, the aqueous particles, which aſcend, will diſpoſe the air to attract the electric fluid in all directions. At this moment the region, which was warmed ſix hours before, will have its nightly dews falling, whoſe condenſation will let looſe that quantity of electric fluid which is ſufficient to ſupply the atmoſphere that is warmed by the meridian ſun, and which, in its paſſage, muſt produce effects altogether adequate to the appearances of the northern lights.

The ſeaſons of the year, in which theſe appearances are moſt rare, and the ſeaſons likewiſe in which they moſt abound, ſeem to corroborate the preceding theory.

In winter, the northern lights, comparatively ſpeaking, are ſeldom ſeen; for the general cold will ſeldom admit of ſuch a gradual and uninterrupted condenſation as that which I have now ſuppoſed. In winter the northern lights are very partial in their extent; for the ſun is ſoon below our horizon, and ſo is the whole region of its poſſible influence.

In ſummer, the northern lights are moſt common, and they are generally the attendants of rain in the courſe of the day, and of an atmoſphere, which becomes clear as the night approaches, or which implies ſuch a change in the weight of the air as will favour the aſcent of whatever aqueous particles may have been accumulated. Again, in ſummer, the auroræ boreales are more to the north than in the winter. In the months of July and Auguſt, they may be often ſeen at midnight; for the ſun is then in its moſt northerly quarter, and even at midnight is not ſo far below our horizon as to preclude the poſſibility that the condenſed part of our viſible hemiſphere may ſupply that portion of electric fluid which its rays are accumulating in a lower region.

Is it, I would aſk, improbable that in the arctic regions, when buried in their long darkneſs, the condenſation produced by increaſing cold ſhould let looſe a vaſt quantity of the electric fluid, which will find its way through the higher parts of the atmoſphere to ſupply the deficiency neceſſarily cauſed by the influence of the ſun in the ſouth? Will not ſuch a paſſage of ſuch an accumulation of the electric fluid account for the almoſt inceſſant corruſcations which enlighten the winters of Greenland and other polar regions?

I give you the preceding theory,

as having this additional circumſtance in its favour, That it is oppoſed by no electric fact with which I am acquainted, and does not appeal to any principles which you have not admitted. It is a cauſe, moreover, that *muſt produce* ſome appearances ſimilar to thoſe of the aurora borealis. But whether it is completely adequate to the elucidation of the peculiarities I have juſt ſpecified, can be determined only by future inveſtigation and experience.

2dly, There are ſtriking varieties in the colours of the northern lights. They are generally a mixture of all the rays; but ſometimes they are of a very deep purple, or perhaps a fine red. The brilliancy of the electric ſpark and of lightning depend (if my obſervations on the light of electricity have any foundation in truth) on their being concentrated, as it were, within very confined dimenſions, and on the degree of that ſeparation of parts which is thus produced. In rarefied air, the portion of the electric fluid is much divided or extenuated, and therefore the eſcape of the light cannot be very ſplendid. The purple colour, I ſhould ſuppoſe to be the effect of a very faint ſeparation of the light.* When the red appears, I ſhould think it to be the effect of the light's paſſing to the eye, through ſuch a medium as is denſe enough to abſorb all the rays but the red.

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* Vid. p. 228. Vol. I.

3dly, The undulation of the northern lights is evidently analagous to the zigzag form of the ſpark or flaſh; for the ſtream of fluid in its paſſage would not be ſtrait, ſave where the conducting particles lie in a direction perfectly ſtrait, which, even in the higher regions of our atmoſphere, cannot always take place. If, therefore, they ſhould be arranged in a line, in any degree curved, the fluid will follow this line, and thus aſſume the waving appearance of the aurora borealis. The flaſhes which attend this phœnomenon are, I think, eaſily accounted for. Provided the rarity of any two mediums be the ſame, I have already ſhewn that they yield electric light in greater or leſs abundance, in proportion as they are bad or good conductors. The heterogenous compoſition of the atmoſphere will juſtify the opinion, that ſome parts of it conduct more than others; the fluid, therefore, in paſſing thro' theſe, will aſſume alternately different degrees of vividneſs or ſplendour. I would obſerve to you, before I finiſh what I have to ſay concerning the northern lights, that I conſider all attempts to determine their height as vague, and all theories on the ſubject as very extravagant. Nothing is clearer than this, That any portion of air may be made luminous by the electric fluid, if charged with a certain additional quantity; and on the other hand, that any portion of the electric fluid may be made to yield its light, provided the rarity of the air through which it paſſes be ſufficiently attenuated. Hence two things are always to be conſidered, before the height of any particular aurora borealis can be determined.—1ſt, The rarity of the air in which it exiſts. 2dly, The portion of fluid that paſſes through the air; for it is evident that at different heights, a variety in theſe circumſtances may produce ſimilar effects, or make ſome auroræ boreales appear within one hundred fathoms of the earth as vivid as others appear which are three miles higher.

METEORS.

If what appears as an undulating flaſh in the northern lights could be concentrated, or confined within ſmaller dimenſions, I have little doubt of its aſſuming the appearance of a falling ſtar.

The following experiment is the fact on which I chiefly ground this opinion:

Into a tube, forty-eight inches long and three-fourths of an inch in diameter, I conveyed as much air as, under the common preſſure of the atmoſphere, would fill two inches in length of the ſame tube. One extremity of this tube I connected by good conductors with the ground, and to the end I faſtened a metallic ball. Through the tube, thus filled with rarefied air, I ſent ſparks of different magnitudes, by bringing the ball within the ſtriking diſtance of different ſized conductors. When the ſparks were ſmall, a flaſh, like that of the aurora borealis, ſeemed to fill the whole tube. When the ſpark was that which might be made to ſtrike through ten inches in the open air, it appeared to ſtrike through the whole length of the tube with all the brilliancy and ſtraitneſs of a falling ſtar. If, however, I extracted part of this air out of the tube, by the aid of the air pump, I could never make the fluid aſſume any form, excepting that of a flaſh. But by exchanging the tube I uſed, for another with a thermometrical bore, and treating it in the ſame manner with the preceding, the flaſh never appeared; but the fluid, in its paſſage, aſſumed all the brilliancy of a falling ſtar.

You will, I think, eaſily trace the ſimilarity of circumſtances obvious in this experiment, to thoſe of the phœnomenon I wiſh to account for. Both take place in rarefied air; both are remarkable for the brightneſs of their light, and for the ſtraitneſs of their direction. That falling ſtars are frequently, if not always, the concentration of an aurora borealis, formed in the manner I have deſcribed, may be inferred moſt plauſibly from their being the conſtant attendants of a very electrical ſtate of the atmoſphere, and from their frequent appearance near that portion of the heavens which is illumined by the northern lights at the time of their appearance.

In a very early ſtage of my electrical purſuits, when prejudice for theory could not influence my mind ſo far as to make me ſee events which did not happen, or to ſee them otherwiſe than with ſome accuracy, I was riding towards Norwich late in the night, when, to the N. E. of the town, I beheld a fine conical ſtream of the aurora borealis. The whole body, every now and then, flaſhed as if an additional quantity of the fluid were thrown into it. At the very ſame inſtant, nearly, I perceived what is vulgarly called a falling ſtar darting from its vertex. This appearance I obſerved twice ſucceſſively. What I have now ſaid, however, I would not wiſh you to conſider as applying more extenſively than to one kind of meteors. Philoſophers have lately had recourſe to electricity for the illuſtration of other phœnomena which bear the ſame name, but the diverſities of whoſe circumſtances are numerous and very conſiderable.

Dr. Blagden has given a very minute detail of particulars relating to a remarkable meteor of this kind which appeared Auguſt 18th, 1783. This meteor was deſcribed differently by different beholders; but their varieties of intelligence Dr. B. aſcribes chiefly to the different directions in which the ſpectators beheld the appearance. All deſcribe its remarkable brilliancy, its train, and the emiſſion of ſparks from its train, which ſeemed in a falling direction. The courſe, moreover, is aſcertained by general conſent, and accounts have been received of its being ſeen as far to the north as the extremities of Scotland, and as far to the ſouth as Nuits in Burgundy. Dr. Blagden computes, from the obſervations of different beholders, that its perpendicular diſtance above the ſurface of the earth *muſt have* *nearly equalled* ſixty miles, where the atmoſphere is 30,000 times rarer than that portion of it which we breathe. Dr. B. adopts, with ſome reluctance, the idea that a noiſe attended it, and that this noiſe was contemporaneous with the appearance: in other words, that *ſound travelled about fifty miles in leſs than a inſtant.* He acknowledges that all philoſophy is directly contrary to the evidence which authenticates this circumſtance; but he has not told us why the evidence of minds, embarraſſed by ſurprize and diſtorted by terror, ſhould be regarded in preference to the whole weight of philoſophy.

I could produce evidences, as reſpectable as thoſe which Dr. B.

relies upon, to prove that *the heat attending the meteor* was intolerable. During the moment of its appearance, I knew one captain of a ſhip, who fancied the heat to be ſo great, that he actually took off his coat and waiſtcoat, to prevent himſelf from being broiled.

There are, I think, ſtrong objections to the whole of that application of circumſtances, on the authority of which Dr. B. has employed electricity to account for this phœnomenon:

1ſt, He ſays that the velocity of the meteor, which he calculates to have moved twenty miles in a minute, is exactly ſimilar to the velocity of electricity. Now, this is ſaying what amounts to nothing, if ſome evidences be not previouſly adduced to ſhew that the electric fluid moved neither more nor leſs than twenty miles in a minute; for between things very great, there may be ſtill a great difference, and though the motion of twenty miles in a minute be conſiderable, it may ſtill be nothing at all, compared with the motion of the electric fluid. I ſaid, *it may be ſo;* I might have ſaid, it *muſt be ſo*, if attention is to be given to the deciſive language of experiment as well as theory on this ſubject. Beſides, the calculation of the velocity is connected with that of the height. If Dr. Blagden be accurate in this particular, I will, on the authority of my own experience, aſſert that it is impoſſible to give the electric fluid the brilliancy of a meteor in air eighty times rarer than that which we breathe, much leſs in air 30,000 times rarer. Even in a confined tube, I have found this impoſſible; much more ſhould I ſuppoſe this to be the caſe, where the electric fluid has an opportunity to expand itſelf on all ſides, or rather, where I ſhould expect that a given portion of it would be ſpread over a very large ſurface. Again, if the hiſſing noiſe, aſcribed to the paſſage of the meteor, were that of electricity, it could not have come from a great diſtance, for its ſound could not have reached far, unleſs we very much change our ideas of its acknowledged ſize. The whole diſcharge of a cloud does not convey a ſound farther than the diſtance of twenty miles; but if the fluid, in its paſſage, flaſhed only, I queſtion much whether the leaſt noiſe could be heard. What then are we to think of the hiſſing of ſuch a ſmall collection of the fluid as that in queſtion, at the diſtance of fifty miles, and in air likewiſe ſo rare; as, agreeably with *the acknowledged principles of philoſophy*, to be incapable of communicating ſound?

Great ſtreſs is laid on the general direction of the meteors from the north or north-weſt quarter of the heavens, and Dr. Blagden accordingly informs us, that "*He conſiders them as maſſes of the elec-*

tric fluid repelled or burſting from the collected body of it in the north, and when they return from the ſouth, he regards them as maſſes attracted towards the ſame accumulation."

In this paſſage a great number of particulars are taken for granted, not one of which has any other baſis than that of *conjecture.*

1ſt, As yet we have ſeen nothing like proof that the phœnomenon is electrical.

2d, The notion that there is a vaſt accumulation of the fluid in the north, is a mere gambol of Dr. Franklin's fancy, indulged, as might be very eaſily ſhewn, in oppoſition to all his former opinions.

3d, If we ſhould not admit what I have ſaid in the preceding Lectures, (to ſhew that the application of a repulſive power is not neceſſary to the explanation of any electrical appearances) we muſt ſtill acknowledge that the repelling principle is not received by electricians on the authority of facts and evidences, but as a plauſible effort of the fancy to ſolve difficulties, which, without its aid, they regarded as altogether enigmatical.

4th, What is there amidſt the known facts or avowed hypotheſes of electricity, to juſtify the ſuppoſition that a ſmall body of the fluid, ſuch as the meteor, if electrical, muſt have been, can be repelled with a force ſufficient to conquer the reſiſtance of a non-conducting medium ſeveral hundred miles in length?

5th, No reaſon is given for the activity of the repulſive power in the caſe under conſideration.

6th, The cauſe that repels in a manner ſo extraordinary becomes an attracting cauſe; but this fancyed exchange of oppoſite characters in the ſame thing is not accounted for by any additional conjecture.

But allowing that the whole of the preceding ſpeculation were true, I ſhould ſtill doubt whether the neceſſity of it be juſtified by the nature of the aſſigned fact, or whether Dr. Blagden be right in aſſerting that meteors generally follow a northern direction. I have had my eyes on the watch, for ſeveral years, to obſerve and examine theſe meteors. That which appeared in 1783, I did not ſee; but I have enjoyed a full view of three different meteors, not leſs remarkable; their ſplendour, their train, and the rapidity of their motion, anſwered both in degree and circumſtance to the deſcription given by Dr. Blagden: of theſe, however, the motion of two were almoſt directly from weſt to eaſt, and one of them, though it ſpread the light of day all around it, diſappeared long before it reached the horizon.

EARTHQUAKES.

Dr. Prieſtley, in his firſt volume of the Hiſtory of Electricity, has given a full account of Dr. Stukeley and Beccaria's theories of earthquakes; and I think, that to a cool inveſtigator of truth, theſe theories muſt ſeem to vie with each other chiefly in their extravagance. Dr. Stukeley's endeavours are for the moſt part employed in conjuring up difficulties againſt the ſuppoſition, that theſe convulſions of nature are the effects of ſubterraneous vapours, and when he has demoliſhed this ſuppoſition, he then takes it for granted, that the agency of electricity muſt be immediately acknowledged. The only poſitive reaſons aſſigned by him are the following:

1ſt, Previouſly to the appearance of an earthquake, which he endeavours to account for, the weather was particularly dry, and therefore very fit for electricity. But electrical ſymptoms are not always the attendants of dry weather, and before the doctor's arguments can be admitted, he ſhould have proved, from proper evidence, that ſigns of electricity were actually obſerved.

2d, Vegetation, according to Dr. Stukeley, is provoked by electricity, and all vegetables, juſt before the great earthquake which was felt in London, were remarkably forward. But is vegetation provoked by nothing beſides electricity? Nay, is it poſſible to deny the agency of numberleſs other circumſtances in producing this effect; and without the moſt deciſive evidence that none of theſe circumſtances operated during the time in queſtion, what attention can be given to Dr. Stukeley's argument? Beſides, this, like the preceding argument, when allowed, only ſhews the probable contemporaneouſneſs of electricity and earthquakes, but is very remote from proving that the one is cauſed by the other.

3d, Remarkable auroræ boreales appeared juſt before the earthquake in London, and ſo they have ſince, without the attendance of an earthquake: as a cauſe and effect, they are therefore by no means neceſſarily connected.

4th, During the night preceding the earthquake, the air was full of corruſcations; *ſo* Dr. Stukeley heard from thoſe who were up at the time. But the corruſcations cannot give aid to his theory, unleſs we prove them to be contemporaneous with the ſhock. The contrary, however, happened; for a black cloud, he ſays, covered the atmoſphere, and though the earth was ſhaken by a moſt uncommon diſcharge of the electric fluid, yet no light ſeems to have been obſerved. *One perſon*, indeed, ſaid that a remarkably great noiſe was heard juſt before the ſhake, which, Dr. Stukeley ſays, was the noiſe of the ſnap previous to the ſhock. This is not the language of an electrician, who would in the preſent caſe have endeavoured to prove, not that the noiſe happened juſt before, but was contemporaneous, or rather poſterior to the convulſion. Beſides, out of that portion of a million which inhabit London, it is remarkably curious, that the authority of one or two perſons only ſhould be produced for the exiſtence of a noiſe, which, if eſtimated from its amazing conſequences, ſhould have awaked half the kingdom.

5th, Dr. Stukeley ſays, that a ſurface of earth, meaſuring 40,000 miles, was affected at the ſame inſtant. He obſerves, that the analogy between this circumſtance and the inſtantaneous effect of lightning is very ſtriking. I have not yet ſeen the evidence neceſſary to eſtabliſh the fact he appeals to, nor do I ſee the analogy; for the preſent hiſtory of electricity does not furniſh us with a ſingle inſtance in which the diſcharge of the fluid is attended with any thing approximating to the extent of effect which he deſcribes. A diſcharge of one cloud covering a great part of Europe, would be a trifle to it, in the concuſſion it produced, though I believe it would be heard and ſeen rather more diſtinctly than that which cauſed Dr. Stukeley's earthquake.

6th, The neighbourhood of rivers and eminences, which are obviouſly the moſt likely paſſages for the electric fluid, are generally, according to Dr. Stukeley, thoſe places in which earthquakes are moſt ſenſibly felt. I would here anſwer, that concuſſion is not the uſual effect of this agent. The noiſe will cauſe a vibration in a houſe which is thunder-ſtruck; but this is the only reſemblance, and is ſuch a faint reſemblance as not to deſerve the name of a ſimilarity to that tottering undulation of an earthquake which has frequently, at the ſame moment, affected Europe, Aſia, and part of America.

The laſt is not the leaſt extraordinary of Dr. S.'s arguments. It ſhews how aſſiduous he was to procure props for his theory, and how far he was from being ſqueamiſh about the nature of what he ſelected. Pains in the back, rheumatic, hyſteric, and nervous caſes, head-achs, cholics, &c. were felt a day or two before the earthquake, juſt as if a general electrization of the human ſpecies had taken place.

Dr. Stukeley does not ſpecify the mode of operation by which the electric fluid produces earthquakes. The boldneſs of an Italian philoſopher has ſupplied this deficiency. Beccaria ſuppoſes that a vaſt body of the electric fluid is lodged in ſome deep repoſitory below the ſurface of the earth, which, when it burſts into the air, will neceſſarily be attended with all the ſingularities of that appearance which he means to illuſtrate. His argument for the exiſtence of a ſubterraneous electrical *mine* is derived from the appearance of electricity at the time of a volcano. But ſurely the change produced in the ſurrounding atmoſphere, at a moment in which it is heated to a moſt amazing degree, when it is loaded with vapours likewiſe, and when its various circumſtances are altogether favourable to decompoſitions and new combinations, will induce the probability of cauſes leſs profound than thoſe ſuppoſed by Beccaria.

The idea that volcanos operate as a funnel to a torrent of electric fluid, guſhing from the bowels of the earth, is followed by an aſſertion, that lightning is always attended by ſhakings of the earth. I have already obſerved, that the noiſe of thunder, like every other great noiſe, is attended by a vibration of ſuch parts as are near it; but the particular effects, attended to by Beccaria, cannot be admitted 'till eſtabliſhed on the beſt hiſtorical evidence, the *reſemblance* of which, in the preſent caſe, I have not been able to diſcover. But I will dwell no longer on the particular reaſonings of particular theoriſts, concerning the analogy between the electric fluid and the cauſe of earthquakes; I will only ſay, that if they are right in their conjectures, they are certainly very wrong in their mode of ſupporting them; they have collected all the circumſtances which they thought favourable, but they have never ſuppoſed the poſſibility of any objections to their theories.

If electricity operates in the production of this effect, it muſt either move a body of the earth, while on its paſſage to the ſurrounding air, or we muſt ſuppoſe the electric fluid to be pent up, and, by endeavouring to burſt its priſon, produce the convulſions of an earthquake. The latter ſuppoſition is ſo wild, as ſcarcely to deſerve any attention; the exiſtence of a body of the electric fluid in the earth, the circumſtances which can induce it to quit its torpid ſtate, and, finally, the reaſon why it ſhould ever burſt ſuddenly, when the conducting nature of the earth might carry it off ſilently, are circumſtances of difficulty which would cruſh a theory much more powerfully ſupported than that to which I allude. Let us, therefore, proceed to examine the firſt ſuppoſition, or that of the fluid's moving a body of the earth in its paſſage to the ſurrounding air. In our examination of this hypotheſis, we ſhall experience the ſame contrariety to eſtabliſhed facts, whether we ſuppoſe the agitation of the earth to be cauſed by a ſudden or by a gradual diſcharge of the fluid. I will here appeal to an experiment to which I have before referred.

If you ſuſpend a balance, ſo delicately as to be turned by the fiftieth part of a grain, and diſcharge the largeſt battery through one extremity of it, not the ſmalleſt motion is perceived. But whether the fluid in this experiment paſs from, or ſtrike to, the extremity of the wire, makes no difference; and, in its circumſtances, what particular is there which is not unfavourable to the theory in queſtion? the balance is perfectly free, and yet unmoved by the greateſt powers of electricity. The earth is attached to a vaſt body of adheſive matter, and yet raiſed ſeveral inches above its level, by an agent which diſcovers in no other inſtance the leaſt ſimilarity of operation. The balance, moreover, is clogged by no weight when the electric fluid paſſes through it without effect; but, according to the theory, millions and millions of tons weight are raiſed by the very ſame cauſe, ſome feet above their natural ſituation.

If it be ſaid, that the charge of a battery is nothing compared with the charge of ſeveral thouſand acres of air, I anſwer, that if a battery produce no *effect* at all, the concluſion is very fair, that 10,000 batteries will not produce any effect; for nothing multiplied into a million, will ſtill equal nothing. But if we admit that an inſtantaneous diſcharge of the electric fluid cannot be the cauſe of earthquakes, let us ſee whether the ſuppoſition of its paſſing in a continued torrent will afford greater aid to the theory.

The balance, which could not be moved by the ſudden diſcharge, is moſt certainly moved by the gradual emiſſion of a very ſmall portion of the electric fluid. But obſerve, that when this is done, it is neceſſary that the ſurrounding air ſhould be ſo ſtrongly electrified, as to erect the hairs of the head and hands; every metallic point, likewiſe, which is near the balance, if viewed in the dark, appears luminous. But none of theſe effects have been obſerved by the thouſands who were ſcattered over the ſurface ſuppoſed to be affected by electricity, during the moment of an earthquake. You will take notice, moreover, that a few grains weight are the utmoſt which artificial electricity can produce in this manner; and if, when ſo ſmall an effect is cauſed, the attendant circumſtances are ſo remarkable, how much more ſo muſt they have been when a torrent of electricity paſſed into the air capable of raiſing millions and millions of tons weight?

But, again, the column of atmoſphere, ſuſpended over the ſurface agitated by the earthquake, can poſſeſs no degree of attractive power adequate to the raiſing ſuch a weight. Action and re-action are equal; a magnet, and a piece of iron of the ſame weight, will meet midway, if ſuſpended in air at a certain diſtance from each other. In like manner, if an electrified body and a feather be ſuſpended, both will be moved; but the ſpaces they paſs through will be inverſely as the weights. If the electrified body were a pound, and the feather a few grains, it is obvious that the motion of the former would be imperceptibly ſmall. But let a portion of the atmoſphere, covering thouſands of miles, be charged, and the difference between its weight and that of the earth, which is agitated, is vaſtly greater than that ſubſiſting between a few grains and a pound weight. You are further to obſerve, that in the one caſe the feather is ſuſpended freely, and ſo is the electrified body; but in the other, the atmoſphere is free to be moved, and the body of earth is attached to every part of its boundary by a very extenſive ſurface of adheſive matter.

I have now given you my reaſons for not aſcribing earthquakes to the agency of electricity: my ideas of the real cauſes to which this phœnomenon may be aſcribed, I ſhall give you on ſome future occaſion.

THE

RELATION OF THE ELECTRIC FLUID

TO

VEGETATION.

That a power, which appears to vary with every minute change of weather, and of courſe with all thoſe circumſtances which contribute to the growth and maturity of vegetable life, ſhould itſelf be active in producing the ſame effect, is an opinion which occurs readily and plauſibly to every philoſophical enquirer. It was formed and ſupported by electricians in a very early ſtage of the ſcience, and at preſent it is amongſt the objects of their buſieſt purſuits and inveſtigations. There is, however, a mortifying difference of magnitude between the ſucceſs that has followed, and the time and labour that have been employed on this ſubject. We are ſo far from aſcertaining the various degrees of electric influence, to which different vegetables may be ſubjected, we are ſo far, likewiſe, from determining the preciſe circumſtances which might cauſe ſuch a variety of influence, that we are ſtill in doubt whether the electric fluid has *any*, or even the leaſt ſhare in the ſyſtem of vegetation.

Mr. Maimbray, of Edinburgh, and the Abbe Nollet, were the firſt who profeſſed to have accelerated the growth of plants by electrifying the pots of earth in which they grew. They have been followed by Mr. Achard, of Berlin, and many other electricians, all of whom have agreed in the reſults of their experiments; nor was the leaſt ſcepticiſm ever prevalent about the ſubject, till Mr. Swankhardt publiſhed the facts which he had learnt from Dr. Ingenhouſz.

Theſe facts, which you may ſee deſcribed in Rozier's Journal, Vol. XXVII. are in all their circumſtances directly oppoſite to the opinions of Nollet, Achard, &c. for they ſhew that the electric fluid, applied in the moſt unequivocal manner, has no influence on the nutrition of any vegetable, not even on the ſenſitive plant itſelf.

Mr. Duvernier took an early opportunity to reprehend the ſcepticiſm of the Dutch philoſophers. He invited them to Paris, and as a reward for their trouble, he promiſed to gratify them with a full and occular refutation of their own errors, by ſhewing them the experiments of Mr. le Duc. He, however, preaches to them a leſſon of modeſty, which, in his opinion, they ſeemed to want, when they oppoſed the authority of certain great names, to which he claims the moſt implicit confidence and the profoundeſt veneration.

Dr. Ingenhouſz replied to the objections which, from various quarters, were directed againſt his friend and himſelf, by publiſhing an account of a very minute and laborious repetition of his former experiments, the reſults of which had confirmed him in his ſcepticiſm; but inſtead of doubting the veracity of thoſe, whoſe reports were different from his own, he endeavours to account for their miſtakes. He ſuppoſes that they would naturally place the uncharged veſſel at a diſtance from the machine which they uſed, and that ſuch a diverſity of ſituation and expoſure to light would be neceſſarily attended with a diverſity of effect. He confirmed his ſuppoſition by experiments made on veſſels in different ſhades in the ſame room.

Thoſe who combated the ſcepticiſm of Dr. Ingenhouſz, had recourſe for arguments to natural as well as artificial electricity. In the garden of a Venetian ſenator, on the Brenta, it was maintained, that a vegetable planted in the ground, penetrated by a conductor, had riſen to a height and ſize much greater than that of ſimilar vegetables planted in the vicinity of the ſame conductor, but not near enough to be influenced by the fluid which *might* have paſſed through it. No abſurdity can ſurpaſs that of an appeal to the preceding fact; for if the electric fluid paſſed *into the ground through the conductor*, how could it influence the vegetable? If it paſſed *out of the ground through the conductor*, the ſame queſtion may be aſked, and the ſame impoſſibility will be apparent, of the vegetable's being influenced by a power which is carried off by a different channel.

Dr. I. to complete the refutation of the objection I have juſt ſtated, laboured through a long ſeries of experiments. He erected conductors near ſeveral trees in the ſame garden; but he never found that any one of theſe grew quicker or higher than others, in whoſe circumſtances there was no difference but that of wanting conductors. Dr. I. made an iron cage, whoſe extremities were points extending into the atmoſphere, and connected by ſeveral metallic communications with a conſiderable piece of ground; but within the limits of this cage, vegetation was not in any degree more flouriſhing than in any other part of his garden.

Dr. I. ſtrengthens the inference he draws from his experiments, by obſerving, that *vegetation is weakeſt when aerial electricity is ſtrongeſt*; and conſequently, that nature varies in this inſtance from its general rules, by employing a power when its influence is leaſt extenſive.

Mr. Van Breda repeated ſeveral of Dr. I.'s experiments, and found his reſults to be uniformly thoſe which his friend had deſcribed.

Dr. Carmoy applied himſelf to the ſubject of vegetable electricity; but his experiments were profeſſedly made in a way different from that adopted by any preceding electrician. He attended with great ſeverity to the poſſible influence of light and temperature, and according to his report, plants grow better in negative than poſitive electricity; but he maintains, that ſoils, impregnated with any electricity, are more favourable to vegetable life, than thoſe that are not electrified. Dr. Carmoy is ſupported by the Abbe d'Ormoy, whoſe experiments are very numerous; they are made on a variety of ſeeds, and with much apparent attention to the effects of light, temperature, and moiſture. In ſome inſtances he has diſcovered, in ten days time, a difference of ten or twelve hours between the firſt appearance of plants that grew from electrified, and thoſe that grew from unelectrified ſeeds. He preſents his reader with a long table, in which the ſeveral varieties of dimenſions are given, which reſulted from a longer or ſhorter application of the electric fluid to different plants.

I would obſerve, that the reſults of the Abbe d'Ormoy's experiments are too uniform to claim unreſerved confidence. In ſome hundreds of experiments, the circumſtances are always favourable to his ſide of the queſtion; though in the experiments of all former electricians, who are advocates for that very theory which he ſupports, the anomalies that take place are ſtriking and numerous. Beſides, he treats all doubts about the influence of electricity in vegetation with too much contempt, and his ſervile reverence for the names which he preſents as favourable to his cauſe, is not very favourable to our ideas of his impartiality as an enquirer.

I feel, however, no reluctance in aſſerting, that the mode of making experiments, adopted by Dr. Carmoy and the Abbe d'Ormoy, and by all thoſe whoſe labours on the ſubject I have had an opportunity to examine, is really ſuch, that the electric fluid cannot poſſibly have the influence they pretend. When I have attempted to convey the electric fluid through a green vegetable, I have conſtantly found that it would paſs over the ſurface, rather than through the body or capillary tubes of the vegetable. This preference of the fluid is peculiarly obvious, when dry wood is uſed, or ſuch ſubſtances as preſerved ſeeds, covered as they are with a fibrous coating, which is almoſt a non-conductor. But obſerve, that the preceding experimentaliſts buried their ſeeds in moiſt earth, whoſe conducting power is vaſtly greater than that of the ſeeds. Nay, ſome of them were abſurd enough to envelope the ſeeds in water, or to wrap them round with tin foil, as if the care they took effectually to guard them againſt the influence of the electric fluid could not be too great.

But allowing that, in oppoſition

to all the principles of the ſcience, ſome ſmall ſhare of the fluid paſſed through the ſeeds, the experimentaliſts in queſtion have ingeniouſly contrived to reduce it, ad infinitum; for they placed the ſeeds in a Leyden phial *charged*, and in moſt caſes *inſulated*. If the inſulation be perfect, the electric fluid becomes as inactive as if no charge were in the bottle. How then is it to influence the vegetation of the ſeeds? But if the inſulation be imperfect, then indeed a ſmall quantity of the fluid is in motion; a quantity, I ſay, ſo ſmall, that if as much goes through the ſeed as through an equal portion of the moiſt earth (which is impoſſible) it does not amount to an infiniteſſimal.

But I will detain you no longer upon the moſt erroneous and unſcientific ſeries of efforts which ever burthened or diſgraced philoſophy. In my opinion, Dr. I.'s ſcepticiſm is not in the leaſt invalidated, and every principle of electricity is ſtrong in favour of the truth of his experiments. He does not pretend to ſay that the electric fluid is altogether incapable of influencing the growth of vegetables; all he aſſerts, is the inſufficiency of every *known* fact to prove the contrary. The power of irritating belongs to the electric fluid, and could it be conveyed through the capillary canals of the young vegetable in the ſeed, it might forward its maturity; but as yet we have not found the method of doing this, nor do we know whether there is any *ſecret* conducting principle to anſwer ſuch a purpoſe amidſt all the unfavourable circumſtances of a moiſt ſoil and ſurface, which, without the aid of a principle hitherto undiſcovered, muſt give an eaſier paſſage to the fluid, than can be afforded by the ſubſtance of the ſeed.

RELATION OF THE ELECTRIC FLUID TO ANIMAL LIFE.

Two views may be taken of this ſubject: 1ſt, That of the electric fluid's influence in forwarding the propagation of life. 2d, That of its influence in aiding or performing the functions of the animal œconomy.

1ſt, The queſtion concerning the influence of electricity in forwarding the propagation of life, is in the ſame undecided ſtate with that of the queſtion concerning its influence on vegetation.

Mr. Achard expoſed a number of

eggs in the hatching heat to a torrent of electric ſparks for ſeveral days ſucceſſively; when this proceſs ended, the embryos were found dead. A German Prince repeated Mr. Achard's experiment, and aſſerted, that after he had continued four eggs for nine days in an electrified ſtate, they were hatched thirty-ſix hours ſooner than four other eggs, which during the ſame time had been ſitten upon by a hen. But the whole of the operation is deſcribed ſo ſlovenly, and managed with ſuch ſovereign diſregard to all diverſities of circumſtances, that, if it had not received the ſanction of royal authority, it would have deſerved but a ſmall portion of reſpect.

Mr. Achard, when he directed his attention to the connexion of electricity and vegetation, renewed his experiments on this ſubject. He placed a certain number of the eggs of a ſilk-worm in a Leyden phial, which was kept in a hatching heat. At the ſame time an equal number of eggs were expoſed to the ſame heat, but not to the influence of the electric fluid. The former were hatched two days ſooner than the latter. We are not told that the experiment was repeated; the ſuppoſition, therefore, that an accidental circumſtance might have interfered, is ſtrong againſt the certainty of the reſult.

I would likewiſe obſerve, that

all the objections I have made to the proceſſes, by which the influence of the electric fluid on vegetables was tried, may be applied with greater force to the preceding facts; for it is impoſſible that any ſparks, or any portion of a charge, can paſs through the ſhell of an egg without breaking it.

2d, One of the moſt brilliant diſcoveries of electricity, is that of the activity of the fluid, in carrying on the functions of animal life.

The inſtances are three, in which it is employed as an inſtrument in the nutritive œconomy; I mean thoſe of the torpedo, the gymnotus electricus, and the particular fiſh, which Lieutenant Patterſon deſcribes in part the ſecond of the 76th volume of the Philoſophical Tranſactions. Mr. Walſh proved that the powers of the torpedo, like thoſe of the Leyden phial, might be extended to any diſtance through a medium of conducting ſubſtances, and that thoſe interruptions, which ſtopped the diſcharge of an electrical accumulation, had the ſame effect on the torpedinous influence; but, as his experiments were made on a ſpecies of fiſh, whoſe power has not yet been found ſufficient to overcome the reſiſtance of the leaſt portion of air, Mr. Walſh could not eſtabliſh his theory by a diſplay of electric *light*.

Mr. Henly was determined by

this circumſtance to doubt the truth of the diſcovery, and was confirmed in his ſcepticiſm by the ſuppoſed impoſſibility of collecting any quantity of the fluid in water. Mr. Cavendiſh has publiſhed a complete refutation of theſe objections, in the 64th volume of the Philoſophical Tranſactions; and in the following year a gymnotus electricus was brought to London, whoſe powers were adequate to the diſſipation of Mr. Henley's doubts. This fiſh conveyed its ſhock through a ſmall interval of air, and in this part of the circuit a ſpark of light, the only required evidence, was made to appear.

The degrees of the power in different individuals of the ſame ſpecies of electrical fiſh muſt vary with their ſize.

The diſcharge of the electric fluid is the means by which their food is procured; but as the creature becomes larger, ſo do the demands of its hunger increaſe, and the ſize of its prey, and conſequently the power neceſſary to deſtroy that prey. We hence meet with great varieties in thoſe deſcriptions of this fiſh, which have been publiſhed by different natural hiſtorians. Their accounts of its effects vary gradually, from a repreſentation of it as a feeble benumbing influence, to that which magnifies it as adequate to the deſtruction of limbs and life itſelf.

From the hiſtory of the gymnotus electricus, given by Drs. Williamſon and Garner, in the 65th vol. of the Philoſophical Tranſactions, we learn, that the ſame individual fiſh has the faculty of varying its power with the ſize of its prey, but that this power depends much on the ſize of the fiſh. That which Dr. Williamſon ſaw, was three feet five inches long. It killed a cat-fiſh, one inch and a half thick. But according to the reports of thoſe who have lived in Surinam, there are ſome of the gymnoti twenty feet long, whoſe ſtroke is attended with inſtant death to the human being.

Of the powers belonging to the fiſh deſcribed by Lieutenant Patterſon, we know little; that which he examined was ſeven inches long, and two and a half wide, and notwithſtanding it was thus ſmall, the ſhock he felt, when he held it in his hand, was ſo ſevere, that it obliged him to quit his graſp.

Mr. John Hunter introduces his anatomical deſcription of the gymnotus with an obſervation ſo extraordinary that I cannot help calling your attention to it. "This animal (he ſays) conſiſts of two parts, viz. the common animal part, and a part which is ſuperadded, or the peculiar organ, which (he adds) does not belong to the animal œconomy of the fiſh." He might, with equal propriety, have ſaid that the jaws, the teeth, and the claws of the lion or tiger do not belong to their animal œconomy, or that they are *ſuperadded parts*; for theſe parts are to the lion, &c. what the electrical organ is to the gymnotus. The circumſtances in which the former are placed, require that they ſhould have teeth, claws, and all their attendant muſcular powers, to catch their prey; thoſe of the latter require that the means, intended for the ſame purpoſe, ſhould be an apparatus which collects and diſcharges the electric fluid.

To determine the peculiarity of circumſtances, which preſcribed the preceding peculiarity of organization, belongs to the philoſophy of natural hiſtory. I cannot help, however, obſerving that the gymnotus and torpedo are able to take in ſo ſmall a portion of prey by one effort at the mouth, and that their motion is ſo ſlow as to require the aid of ſome inſtrument, which would be fatal at a diſtance, and would altogether deſtroy before they begin to feed. This operation likewiſe, ſimilarly to the venomous organs of ſerpents, may have a connexion with the power of the gaſtric juice, or the peculiar degree of digeſtive faculty which belongs to the animal.

Two things remain to be conſidered; 1ſt, The *mode* of operation by which the effect is produced. 2d, The peculiarities of effect which depend upon the peculiarity of the mode of operation.

1ſt, It is evident that the ſhock is conveyed by making the water the diſcharging medium between two parts of the body, which are in oppoſite ſtates, and it is clear, from Mr. Hunter's diſſection, that there are two diſtinct organs concerned in the operation. We may preſume, therefore, that one of theſe organs accumulates, while the other loſes a portion of the fluid, or becomes negative. I can, likewiſe, eaſily conceive that theſe oppoſite effects may be produced by a ſudden flow of any circulating fluid into one organ, while a contemporaneous, but an equal evacuation takes place in the other. When this proceſs is completed, a ſubſequent filling of the empty, and evacuation of the full organ, by removing the cauſe, will produce the diſcharge of the accumulation.

I have recurred with frequency to the principle on which the preceding explanation is founded. Any extenſive contact of two different bodies muſt cauſe a change of attractive force, and in the caſe of electrical fiſhes, this may be effected, not merely by a ſudden flow of blood or any other circulating fluid into a certain part of the animal organization, but by a ſudden *condenſation* of the parts in one organ, and an equal and contemporaneous rarefaction in the other.

You will obſerve that I have ſpecified two modes of operation, but which of them prevails in the gymnotus and torpedo, cannot be determined; for Mr. Hunter has not deſcribed the texture of the organs with a fulneſs adequate to ſuch a deciſion. He ſimply ſtates, that they are compoſed of ſepta and membranous intervals. The ſepta he deſcribes as walls or butments for the membranous intervals; but he does not ſay whether the membranous intervals are vaſcular or otherwiſe, nor does he ſeem to have examined their tenſion or capability of being made to occupy greater or leſs room by certain exertions of the animal.

Let us, however, ſuppoſe that by either of the deſcribed changes an accumulation has taken place; this queſtion will then immediately occur. How is the charge prevented from paſſing through the body of the animal to the correſpondent negative? I would anſwer, that a ſtratum of adipoſe ſubſtance, lying between the two organs, would be fully adequate to ſuch a purpoſe; for the non-conducting property of the oily particles would preſerve any accumulation, till diſpoſed of by the wants of the fiſh.

From Mr. Hunter's diſſection it is clear, that a conſiderable partition of fatty ſubſtance ſeparates the organs, whoſe conducting or membranous parts, diſperſed through the oil, muſt be ſo ſmall in dimenſions as to take away but little from the ſudden accumulation made by the animal; this is clear ſrom our experiments on water and other bodies, which are better conductors than membrane, but which, when diſperſed through capillary tubes, become incapable of conveying the ſhock.

The peculiar mode of operation which I have deſcribed, will ſuggeſt to us what I have repreſented in the preſent caſe as the ſecond object of conſideration, or thoſe peculiarities of effect which depend on the mode of operation, and to which there are ſtriking ſimilarities in the hiſtory of the electrical fiſhes. When I attempted to charge two ſurfaces, which were divided from each other by a ſtratum of oil, I could procure no ſhock, unleſs the diſcharge was made while the accumulation was continued, or while it was preſerved by ſucceſſive additions of the fluid, or by the leaſt interruption in the ſtream that ſupplied the ſurfaces, the collected quantity was immediately diſperſed, owing to the imperfect inſulation of the oil. Indeed, the higheſt charge produced in theſe circumſtances was very trifling; but theſe circumſtances very nearly reſemble thoſe of the electrical organs of the fiſh, and we accordingly find that their charges and diſcharges are ſo contemporaneous, that ſeveral of them are made within the minute.

We find, moreover, that their greateſt height of charge is ſcarcely able to penetrate the leaſt interval of air. This is the peculiarity that determined nature in giving its peculiar ſize to the organs; for there is no other method of making a low charge powerful, than that of ſpreading it over a very extenſive ſurface. By multiplying the number of jars in a battery, a charge may be obtained, ſufficient to melt a conſiderable quantity of wire, and yet not able to penetrate thro' one-eighth of an inch of air. Nature, conſequently, has ſupplied the torpedo and gymnotus with their requiſite force, by giving a moſt extenſive ſurface to the part in which they collect the charge, and as they reſide conſtantly in water, there can be no combination of circumſtances in which a charge, ſufficient to conquer a great reſiſtance of air, would be neceſſary to their wants.

In conſequence of the facts we have juſt deſcribed and endeavoured to explain, it was fairly inferred, that if the electric fluid could be employed in the animal ſyſtem, and exerted to a degree capable of ſuch power as that exercifed by the torpedo and the gymnotus, then the probability was conſiderable, of its frequent activity amongſt the numerous expedients which muſt belong to the complicate ſtructures of all organized matter. Indeed, the diſcovery of Mr. Walſh was received like the introduction of a new principle into natural philoſophy, the poſſible extent of whoſe application was regarded as boundleſs. It gave birth to ſome of the boldeſt conjectures, and there were few phyſiologiſts who did not conſider it as promiſing a ſpeedy developement of every myſtery belonging to the nervous influence and muſcular contraction. Fancyful, however, as the ſpeculations ſeemed to be in the opinions of ſome philoſophers, they have, notwithſtanding, been very much countenanced by the experiments of Signor Galvani.

I will give you a brief account of the facts which he has diſcovered, and of others, which are merely the reſults of repeating his proceſſes with ſome variation.

A ſuperficial recollection, however, is previouſly neceſſary, of the general diſtribution of the nerves. The ſources from which all of them originate, are cavities of the head and of the back-bone; through certain perforations in theſe, " they paſs in various directions towards the different parts of the animal ſtructure. Of their firſt and great ramifications, ſeveral have no connexion, otherwiſe than by the common trunk from which all ſpring; and when each great ramification is divided into hundreds that are ſmaller, and theſe ſmaller ones again into thouſands that are more minute, the ſame characteriſtic is ſtill obſervable. By following their courſe backward from that ſtate in which they are ſcarcely viſible by the eye, you will find that any influence, directed according to this courſe, would paſs by myriads of nervous ramifications without entering them.

Let us now ſuppoſe that the nerves, diſtributed as I have now deſcribed, are partial conductors of the electric fluid, whoſe property it is to irritate or to increaſe their powers of action; the conſequences muſt be the following:

1ſt, The electric fluid, in its paſſage, will operate on thoſe nerves only which form a circuit, and the action which expreſſes itſelf by contracting the muſcles which the nerves enter, will not affect a great number of other nerves which are, as it were, ramifications of ramifications. If a ſtrong ſpark be received at the elbow, and conveyed to the little finger, the contraction will not affect the other fingers. If the ſpark be conveyed into the ſpine, and the metallic ſubſtance which is to convey it out of the ſyſtem be applied to one leg, that leg only will be ſhaken.

2d, Let us ſuppoſe, that inſtead of applying artificial electricity to the nerves, a quantity of it were collected, or a poſitive charge formed by a certain alteration of circumſtances, at the extremity of a nerve, or any given part of it; a contraction would neceſſarily take place along the whole extent of the nerve or nerves forming the circuit through which the charge paſſed to its correſpondent negative.

Signor Galvani has diſcovered that ſuch an accumulation is made by applying the ſurfaces of metals to thoſe of certain animal ſubſtances, and that this accumulation is again diſcharged by forming a communication with the ſame, but more powerfully with a different metallic ſubſtance between thoſe ramifications of the nerves which are neareſt the metallic ſurface, and other ramifications which are more diſtant parts of the ſame nerve.

The contraction which is the effect of the foregoing proceſs is beſt produced by applying gold, ſilver, or tin, to the animal ſubſtance, and by forming the communication with zinc. That the contraction is cauſed by the electric fluid, is proved by applying Mr. Bennet's electroſcope as a teſt. If the ſpine of a frog and the nerves of one of its feet be laid bare, by placing a piece of tin on the one, and connecting it by a piece of zinc with the other part, the whole leg is inſtantly contracted.

If the preceding experiment be made on the limb of a dead frog, the application of the metals, as directed, will give it all the ſeeming convulſions of life; the increaſed irritability, however, cauſed by this proceſs, is ſuppoſed to be moſt deciſively ſhewn by the following fact:—Place a half-crown piece *under* your tongue, and a piece of zinc *upon* your tongue; then bring them, while in this ſituation, into contact, and you will inſtantly perceive a diſagreeableneſs of taſte, which is aſcribed to the increaſed action of thoſe nerves on which that ſenſe depends. If the tongue be cloathed with tin foil, and a piece of ſilver be introduced within the noſtril, by forming a metallic communication between the tin and the ſilver, provided the operator be in the dark, and his eyes kept cloſed, a luminous flaſh will ſeem to ſpread over his eyes, which is aſcribed to the activity of the electric fluid in its paſſage over ſome ramifications of the optic nerve.

The difficulties are great which embarraſs our attempts to account for the preceding facts, and for the ſeveral varieties of appearance which have been found to attend certain diverſifications of their circumſtances. They are ſuppoſed, amongſt other ſingularities, to preſent us with a caſe in which the electric fluid is collected and rendered active by a cauſe hitherto undiſcovered. Some philoſophers have aſcribed the whole to "a peculiar but undefined power which belongs to the nerves, and which they poſſeſs, and exerciſe while forming a part of the living body, ſo as by its means to produce all the phœnomena of nervous influence and muſcular motion."—We gain nothing by acceding to this indeterminate and hypothetical language: it does not anſwer one out of the numerous queſtions which ariſe from the facts it is meant to explain; it leaves us altogether in the dark about the neceſſity of applying metal before the nerves are affected, and more particularly about the conſequence of forming the circuit with one metal, while the electricity is produced by another. I would, however, obſerve that the nerves do not ſeem to have any concern in the firſt excitation of the fluid; for if a piece of fleſh be placed between the nerves and the tin foil, or if the tin be placed on water in contact with the nerves, the appearances are as deciſive as when the tin is immediately applied to the nerves* themſelves.

It is well known that the preſſure or cloſe contact of any two different bodies will be attended with ſigns of electricity. The evaporation of a ſingle drop of water; the application of a piece of cloth, not exceeding one inch ſquare in dimenſions, to the ſurface either of an electric or non-electric; the blowing of a few particles of duſt againſt a metallic ball: all theſe inconſiderable changes of circumſtances are ſources of electricity. But if cauſes, apparently ſo trifling in their influence, are attended with effects ſo deciſive, why may not the application of tin foil, or any other metallic ſurface, to a fleſhy ſubſtance, be attended with ſimilar effects? The cauſe is exactly the ſame in all the inſtances I have ſpecified; and if it be admitted in the laſt, or in the caſe in queſtion, as ſufficiently powerful to produce electricity, our difficulties will be reduced to that of aſſigning the particular mode of its action. I would not preſume to aſſert what the mode is, I would only aſk whether it *may* not be the following:

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* Vid. Monro's Exp. on the New Syſtem, p. 24. Prieſtley's Heads of Lectures, p. 166.

When the tin foil is applied to the nerves, or the adjoining fleſhy ſubſtance, an increaſed or diminiſhed attractive force brings to the points of contact, or ſeparates from them, a certain portion of the electric fluid, which is either taken from or conducted away by the adjoining bodies.

I ſuppoſe the caſe I have deſcribed to be exactly ſimilar to that of the following experiment.—Place a ſheet of dry paper, heated ſo as to be ſcarcely tolerable to the hand, on an inſulated metallic plate; then rub its ſurface with a piece of Indian rubber: in a few ſeconds the adheſion of the two ſurfaces will be ſuch as, with difficulty, to admit of ſeparation, and no electrician will doubt that this adheſion proceeds from the loſs or addition of a certain quantity of the fluid, cauſed by the contact of the different ſurfaces.

While the tin is in contact with the fleſhy ſubſtance, no ſigns of electricity are diſcoverable: ſo, while the paper is in contact with the metal, no ſigns of electricity can be diſcovered by the moſt ſenſible electroſcope; but by bringing another piece of metal to participate in the influence of the tin foil on the fleſhy ſubſtance, and at the ſame time to extend its actions to both extremities of the nerves, then a new combination of circumſtances takes place, a new change of attractive force, and inſtantaneouſly the activity of the electric fluid, in its paſſage to or from the parts of contact, is immediately diſcovered by the contraction of the nerve.

In like manner, if you ſeparate the paper and the metal, or place any different body on the upper ſurface of the paper, or bring it into the ſame contact with the paper, or produce any change of the circumſtances on which the attractive force depends, a portion of the electric fluid is immediately ſeparated, ſo that a ſpark, half an inch long, will ſtrike into any conducting body which is applied to the inſulated metal.

You will, I hope, remember, that the preceding elucidation of the curious facts, diſcovered by Signor Galvani, is propoſed to you merely as a ſtimulus to farther inveſtigation. Indeed, no part of the ſcience of electricity holds forth a more inviting purſuit than that of its connexion with the animal œconomy, in which the poſſible agency of the electric fluid appears to be that of boundleſs extent and the moſt varied application.

The moſt abundant, if not the only, ſources of this agency in nature, are thoſe cauſes which ſeparate and compound different ſubſtances; but the animal ſyſtem is a multitudinous collection of inſtances in which ſuch cauſes operate. — The glands, the exhalant and the abſorbent veſſels, are every inſtant mixing different compounds, or ſecreting their ſeveral parts, and according to the general law of nature, they muſt be perpetually collecting or demanding new quantities of the electric fluid. But the effects of the preceding animal proceſſes are increaſed by the various motions of the ſyſtem which depend upon the will. A new flow of blood, propelled into any part by increaſed action, forms new contacts or ſeparations, and of courſe a new cauſe of electricity.

The poſſible means, however, with which the animal ſyſtem is furniſhed for preſerving or inſulating any collection of the fluid, is adequate to the numerous cauſes by which it may be produced.—There is ſcarcely any important organ that is not ſurrounded with animal oil. This is particularly the caſe with moſt of the glands, and with every diſtinct muſcular fibre. Indeed, the whole muſcular ſyſtem is a collection of the moſt perfect means for electrical excitation and accumulation. Its vaſcular parts are conſtructed for the admiſſion of a ſudden flow of blood, ſo as to come in contact with the largeſt ſurface poſſible, and the interſtitial oil, which ſeparates each diſtinct fibre from the next to it, confines the collected electric fluid, ſo that it muſt be carried off by the nerves which belong to thoſe parts, and which, when they conduct, are immediately contracted.

Could we but eſtabliſh the ſpeculation which I am now hazarding as having a faint appearance only of probability, we ſhould be able to explain a great number of facts which have been hitherto conſidered as inexplicable, many varieties would be traced to the quantity of adipoſe membrane, and many changes of health and ſtrength would neceſſarily proceed from the diverſities of its quality as well as its bulk. The ſtate of the vaſcular fibre would open another ſource of changes, ſo would the various degrees of action or force by which the circulating fluids are propelled into it. There is, however, no limit to that theoretical proſpect which I have now opened to your view, nor to that ſeries of labours which muſt precede every ſtep you take, if you mean to advance with proper ſatisfaction or the leaſt degree of certainty.

DIRECTIONS

FOR THE

CONSTRUCTION

OF AN

Electrical Apparatus.

IN the conſtruction of your electrical apparatus, it ſhould be the firſt object of your endeavours to avoid all waſte of power, or that extravagance in the conſumption of time, of talents, and of means, which takes place when your movements are too ſlow, too numerous, or too complicate; when your ingredients are unneceſſarily expenſive; when you operate on too ſmall a ſcale, and thus fall into errors which are corrected only by a repetition of efforts, or when, by working on too large a ſcale, each reſult becomes the fruit of exceſſive labour.

An experimentaliſt, by avoiding all waſte of power, or by cultivating that ſkill which does neither too much nor too little, eſcapes from ſome of the moſt tireſome incumbrances of his purſuit. His apparatus will be ſimple, and when his reſults diſappoint him, he will have but few parts to examine as the ſources of his error. His apparatus will be cheap; and while his own powers are thus enlarged, he increaſes the probability that other men's endeavours will co-operate with his own. His apparatus will be permanent, and the repetition of innumerable and tedious labours will be unneceſſary. His apparatus will be eaſily underſtood, and conſequently its poſſible improvements will be the more obvious, and the more likely to be realized. In fine, like all the expedients of wiſdom, thoſe which he applies to his own peculiar circumſtances will be fully adequate to their demands, and will ſquander nothing which may be uſeful in future exigencies.

The common ſource of artificial electricity is friction: it ſhould therefore be our firſt buſineſs to determine the nature and form of the ſubſtance, by the rubbing of which the fluid is moſt conveniently accumulated.

Glaſs is preferred for the following reaſons:

1ſt, It is the moſt durable of all electrics.

2d, It may be moulded into any required ſhape.

3d, It is the lighteſt of all electrics.

4th, Though it attracts moiſture very rapidly, yet moiſture is removed from it more eaſily than from any other body. Aqueous particles enter the ſubſtance of other electrics, but are confined to the ſurface of glaſs, and therefore are inſtantly wiped off with a dry linen towel.

Some electricians have preferred glaſs of a ſpherical form; but the cuſhion that is ſuited to it muſt participate of that form, and this ſingle circumſtance renders the application of preſſure difficult and limited. Beſides, the ſilk that is annexed to the cuſhion can never be cut ſo as to lie exactly cloſe to the glaſs.

A ſpherical form has been recommended by the complete inſulation on the upper part of the ſphere, which is the conſequence of making it revolve on an axis perpendicular to the ground. But when the ſphere reſts on ſuch an axis, it is perpetually looſening, for the preſſure of the cuſhion is applied beyond the fulcrum, and of courſe prevails in a ſhort time over the adheſion of the ſtrongeſt cement.

Plates of glaſs are in faſhionable uſe on the Continent, and the effects of them are deſcribed as prodigies in the foreign journals. But I obſerve that the criterion by which they judge is the power of a charge communicated to a given battery. There is, however, no immediate connexion between the charge of which a battery is capable, and the power of the glaſs that is rubbed; for there is ſcarcely any degree of accumulation which you may not collect with a ſmall glaſs cylinder, provided you multiply your jars, and employ time enough to charge them. It is certain that no reaſon can be aſſigned for believing that a plate will admit of greater excitation than glaſs of any other form. Whatever advantage does belong to it muſt proceed from having the friction applied to both ſurfaces at once, or from applying the cauſe of excitation in ſuch a manner that it may act upon the thinneſt poſſible ſurface of the electric. This advantage, however, muſt be imaginary, for it is found that a ſolid body may be excited as powerfully as the thinneſt glaſs. If any ſuperiority ariſes from difference of preſſure, it cannot belong to the plate machines; for all thoſe which have been hitherto conſtructed, are incapable of that preſſure which may, without the leaſt danger or inconvenience, be applied to the cylinder.

The poſitive objections to the uſe of glaſs plates are numerous and weighty:

1ſt, The contrivances neceſſary for their inſulation are very complicate.

2d, They are not only in themſelves more expenſive than glaſs of any other form, but they are fitted up with greater difficulty.

3d, They are faſtened to their ſupport at the centre, but the applied preſſure operates near the periphery ; it follows, therefore, that if much force be uſed, the glaſs will looſen, and get out of order, in defiance of every care to faſten it with a ſtrong and durable cement.

4th, Plate-glaſſes are of all others the moſt expoſed to injury, and when their ſize is conſiderable, the magnitude of this injury makes it a ſerious calamity, even to the moſt wealthy electrician. Beſides, the injury is cauſed by the ſlighteſt blow, or by a trifling inaccuracy in fixing them at the centre ; for the leaſt unequal preſſure is apt to crack them.

5th, They do not admit of that renewed application of the amalgam which is ſo eſſential to the perfect excitation of an electric.

6th, The plate, and the inſulations neceſſary for it, take up too much room.

I prefer cylindrical glaſſes open at both ends, for the following reaſons :

1ſt, By admitting of ſupports at both ends, they are capable of being fixed with perfect ſecurity, ſo as to bear any preſſure or ſhaking, to which they may be expoſed during the long operations of a laborious experimentaliſt.

2d, The cuſhion and the ſilk may be fitted perfectly cloſe to their ſides, and during the operation, freſh portions of amalgam may be applied to them, without any inconvenience to the operator.

3d, The conductor may be placed altogether within the atmoſphere produced by cylindrical glaſſes. This is a great advantage; it can be eſtimated only from experience, and does not belong either to the plate or the ſphere.

4th, Machines conſtructed with cylindrical glaſſes take up the leaſt room. Even with conductors, which are fixed parallel to their ſides, they may be fixed on a moderately ſized table, and preſerved in a ſmall cloſet, ſo as to be readily brought out when wanted.

I would adviſe you, when you purchaſe a

cylinder, to get it blown ſo that it may be as light as poſſible ; not that I regard one weight as more favourable than another to the production of electricity, but the heavier your machinery is, the more likely is it to be injured during the numberleſs motions which attend the uſe of it.

The ſurface of glaſs is frequently covered with little protuberances and inequalities, whoſe points and edges tear the ſilk, accumulate dirt, and multiply the neceſſities of cleaning. To avoid theſe inconveniencies, you ſhould find out the time at the glaſs-houſe, in which the workmen open their pot, for the roughneſſes I allude to are the effects of the vitrified matter's imperfect fuſion which takes place when the pot is nearly full or very low. The beſt cylinders are blown only when the pot is half emptied, and this is the caſe on Wedneſdays and Thurſdays, in the London glaſs-houſes.

The necks of your cylinder ſhould be wide enough to admit the hand, for a condenſation of vapour is frequently formed on the internal ſurface of the glaſs, and if you are able to wipe it off with linen very dry and hot, you avoid the neceſſary but very dangerous trouble of removing it by a covering of ſpirits of wine and Spaniſh white, laid on, and rubbed off with a rag tied to the end of ſuch a bent wire as the narrowneſs of the neck will admit.

I have found that no cylinder, leſs than fourteen inches in diameter, will enable an operator to uſe a battery with a proper degree of power, without immenſe loſs of time and labour. The length of ſuch a cylinder ſhould not be leſs than twenty-one inches, your arm in its circumvolutions will otherwiſe get ſo far into the atmoſphere of the conductor, as to take off a very conſiderable portion of the accumulated fluid.

The caps of cylinders are generally made of wood very well dried; for the leaſt remaining ſap or moiſture will be evaporated by the warm cement, and the contraction thus cauſed, in addition to that proceeding from the expoſure of a new ſurface of wood, is adequate to the deſtruction of the cylinder. The hard tropical woods are generally uſed, but there is not one of them which I have been able to uſe without injury, and I am clearly of opinion, that a very light braſs cap is the beſt that can be ſelected: the rejection of it, on account of its ſuperior conducting power, is abſurd; for the inſulation depends on the diſtance of the cuſhion from the cap, and provided that diſtance be well managed, what the cap is compoſed of is of little conſequence. Beſides, the dryeſt wood is not a better INSULATOR than metal, and it is much more likely to have points on its ſurface than braſs well poliſhed and varniſhed. One part of the braſs cap ſhould be a ring cloſely fitted to the neck of the cylinder, and having an orifice equal to the admiſſion of the hand: the greater the ſuperficial dimenſions of this ring, the better, for the more extenſive is the adheſion of the cement. A head or lid of braſs, carefully cleared of all edges, ſhould be ſcrewed on the ring, and into the centre of this lid, on the one ſide the handle, ſhould be ſcrewed, and on the other the pin on which the cylinder turns.

At one time part of the handle was made of glaſs. This was very abſurd; for the glaſs does not ſhorten the interval which is moſt favourable to the eſcape of the fluid, or that which ſeparates the hand from the conductor, when it becomes oppoſite to it in its circumvolutions. The ſame reaſon ſhews the abſurdity of inſulating that end of the cylinder to which the handle is fixed. The inſulation of the other end is rendered proper by the particular poſition of the conductor.

An unneceſſary multiplication of inſulators becomes a ſource of conſiderable annoyance to an experimentaliſt, who purſues the ſcience with any degree of ardour; for when the motion is great and of long continuance, the beſt cement will give way, and no interruption can be more teaſing than that of being ſtopped to repair a damage in the middle of a proceſs.

The CUSHION is generally made of hair covered with leather. It ſhould be as hard as the bottom of a new-made mohair chair, and it ſhould be formed ſo as to be in perpetual contact with the ſides of the glaſs when it revolves. The length of the cuſhion ſhould never exceed one-third the length of the cylinder, for one end of the conductor muſt always approach two or three inches nearer the hand than the cuſhion; if, therefore, it exceed the above dimenſions, the inſulation muſt be very incomplete.

When the ſilk is omitted, the poſſible charge of the conductor, comparatively ſpeaking, is ſmall. The ſilk ſhould be as wide as the cuſhion; it ſhould be ſewed upon a wire bent at both ends, which ends are preſſed into holes made for their reception on the upper edge of the wood to which the cuſhion is faſtened. The ſilk ſhould be equal in length to half the circumference of the cylinder, and its edge ſhould terminate where the points, affixed to the conductor, touch the glaſs. Formerly, great pains were taken to prepare the ſilk; but I find that a piece of common taffeta, without any preparation, anſwers every purpoſe. It ſhould be placed ſo that the motions of the cylinder do not croſs the grain, it will otherwiſe be very ſoon worn into threads.

The cuſhion ſhould be fixed on an elaſtic ſupport; for no cylinder is perfectly even, and unleſs there be ſome little giving-way, a fracture or injury of one kind or another will ſpeedily take place.

The length of the conductor, which I have found to be moſt convenient, is equal to that of the cylinder, including the necks. Its diameter is nearly one-third of that of the cylinder. It is made of copper, which is better than tinned iron, as having a ſmoother and more durable ſurface. Its extremities are ſpherical lids or covers, ſo fitted to the cylindrical part of the conductor, as to be taken off and put on without difficulty. Different parts of the conductor are penetrated by copper tubes about two-thirds of an inch in diameter, which enable the operator with eaſe to connect the different parts of his apparatus.

In Mr. Nairne's patent machine, the receiving points are fixed; but it is moſt convenient to be able, at pleaſure, to change their poſition; for the general power of inſulation belonging to the machine depends much on this circumſtance, as the ſmalleſt variety in the poſition of the points will frequently cauſe the greateſt difference in the degree of accumulation. Agreeably to a method which I learned from Mr. Read, I fix my points into a number of little braſs rings; theſe turn round upon an axis, the extremities of which are bent and forced into two holes in the ſide of the conductor. The points are thus made to reſt upon the glaſs, and conſequently to be in perpetual contact with it, without cauſing the leaſt impediment to its motion.

The different receipts for amalgam are almoſt as numerous as the different experimentaliſts who have purſued the ſcience. I am determined by my own experience, which agrees with that of Mr. Brooke, to prefer the following which does not differ much from that recommended in the Philoſophical Tranſactions.

Take one part of zinc, place it over a fire in an iron ladle. When the ladle is nearly red hot, put a little tallow on the zinc, and it will immediately be melted; while in a ſtate of fuſion, pour upon it four parts of quickſilver: this ſhould be done very cautiouſly, the quickſilver will otherwiſe be diſperſed by the ſudden application of the heat, and loſt. I never mix any greaſe with this amalgam; the method of uſing which will be underſtood from the directions which I ſhall now give for the excitation of the cylinder.

You ſhould begin your work with removing all duſt from your apparatus, the table, and the adjoining floor. You are next, with a warm dry cloth, to wipe all your inſulators. In caſe your cylinder ſhould be rendered greaſy or dirty by former operations, you ſhould clean it with ſpirits of wine. Indeed, the degree of your excitation will depend altogether on the care you exerciſe to keep the glaſs free from filth and moiſture.

When the cuſhion is cloſely preſſed to the ſides of the glaſs, and while the cylinder revolves, apply your amalgam to the uncovered part of the glaſs: this may be done by previouſly faſtening it to a piece of leather with bees-wax or glue; or it may be done by applying a hard piece of it, ſcraped with a knife, ſo that its ſurface may be in contact with that portion of the cylinder againſt which the hand preſſes it. If the fluid, in a few ſeconds after the application of the amalgam, is not abundantly collected, ſpread a little oil on the palm of your hand, and let it, thus greaſed, juſt touch the cylinder as it moves round ; the effect will be inſtantaneous, as will be apparent from the increaſed reſiſtance of the cuſhion and the numerous torrents of the fluid which will paſs from the edge of the ſilk to your hand. This operation may be repeated without trouble, as often as you want to revive the languiſhing excitation of the cylinder. It is of ſome conſequence that the amalgam ſhould not be partially calcined, which is the caſe when its ſurface has been expoſed to the air, or when any part of it has been rubbed by lying between the cuſhion and the cylinder. When the proper degree of excitation prevails, an interval of three ſeconds does not ſeparate the ſeveral brilliant ſparks which ſtrike in ſucceſſion from the points of the cuſhion.

I have now deſcribed every part of the apparatus that is neceſſary for the accumulation of the fluid. There are two ways in which its force may be directed or applied, when accumulated; you may convey it as a ſpark taken immediately from your conductor, or you may collect it on the ſurface of glaſs, and examine the peculiarities of its effects in paſſing from the poſitive ſide to the negative. When the ſpark is uſed, that machinery is beſt, which admits the greateſt accumulation and the moſt frequent diſcharge of it in the ſhorteſt time; for this purpoſe, the dimenſions of the cylinder and the conductor ſhould be very large. If the reſpective diameters of the cylinder and conductor be twenty-one inches and ten inches, if their reſpective lengths be two feet and a half and ten feet, a *perpetual torrent* of the fluid will paſs from one ball of two inches in diameter to another, through an interval of one inch and a half of air. If the preceding diameter and lengths be diminiſhed, you will find a correſpondent change in the ſounds and in the rapidity of the ſucceſſion of ſparks.

In caſe you ſhould uſe a chemico electrical apparatus, you will find it neceſſary to employ this mode of directing the fluid, on the greateſt ſcale poſſible; for in ſome inſtances a ſmall change is produced only by millions of powerful ſparks, and conſequently the ſize of your machine muſt cauſe a difference of ſome hours labour in the courſe of one day. Your communicating and receiving balls ſhould never be leſs than two inches in diameter; you are moſt ſecure of obtaining the greateſt force of the ſpark when their diameter is three inches. The cheapeſt balls are made of wood, and covered with tin foil; but the tin foil is perpetually looſening, and cauſing inequalities; the toil, therefore, is perpetually recurring, of finding out and preſſing down the points which riſe on their ſurface. Light copper balls are preferable, but they are dear, and without great care, when much uſe is made of them, they are apt to be bulged and ſpoiled.

For the convenience of adapting the ſituation of your balls to the ſituation of the body on which you make your experiments, it will be neceſſary that you ſhould have tin tubes of different lengths; their diameters ſhould not be leſs than half an inch, and the edge at which the ſolder is put on, ſhould be carefully filed down. Copper tubes are better, but they are very expenſive. For the purpoſe of faſtening the tubes to the conductor, Meſſrs. Nairne and Blunt have contrived a joint, which admits of being moved in two different directions; to one part of this joint is fixed a piece of wood or braſs, which enters the conductor; to the other is fixed a piece of braſs or wood, on which the tube is ſlipped or ſcrewed, having at its extremity the receiving or communicating ball. When experiments are to be made with the charges of glaſs, you may uſe a ſingle phial or a battery.

The beſt form of the ſingle phial is that of a perfect cylinder, having its mouth wide enough for the introduction of the hand. On a glaſs thus formed, the tin foil is laid, and the duſt, that is apt to collect on the inſide, removed without any trouble.

If the glaſs be thinner than one-eighth of an inch, the phial will bear no conſiderable height of charge. The wire, through which the charge is communicated, ſhould not be leſs than one-fourth of an inch in diameter; it ſhould terminate in a metallic ball, one inch at leaſt in diameter.

If the phial be intended for frequent change of ſituation, the charging wire ſhould be faſtened ſo as to be always ſteady in the centre of the phial. Some perſons uſe for this purpoſe a piece of wood, which fits the rim of the glaſs like a lid; but the length of inſulation, which ſeparates the tin foil from the charging wire, is thus leſſened, and conſequently the eaſe with which the fluid paſſes from the inſide to the outſide facilitated, ſo that the phial is perpetually loſing part of its contents, and the time neceſſary to fill it conſiderably lengthened. The beſt faſtening is that which is applied to the wire below the edge of the tin foil.

When the phial is not to be moved from the ſituation in which it is charged, the wire may be faſtened to the conductor; the glaſs may then be removed without difficulty for the purpoſe of cleaning it, and for a variety of experiments on the ſurfaces of bodies, which you have ſeen during the courſe of theſe Lectures.

The BATTERY is a collection of phials, ſo connected that the force of any number of them may be conveyed at the ſame inſtant through the ſame circuit.

The following are eſſential requiſites in the conſtruction of a battery:

1ſt, Its connecting wires ſhould be perfectly free from all edges and points.

2d, The connecting wires ſhould be eaſily moveable, ſo that when accident has leſſened the number of phials, the wires may be reduced ſo as to correſpond with the remaining quantity of glaſs.

3d, The phials ſhould not be crouded; for in ſuch a caſe, ſhould neceſſity oblige us to uſe phials of different ſizes or heights, the tin foil of the higher ones being in contact with the uncoated glaſs of the lower ones, will leſſen their inſulation.

4th, The ſize of the phial ſhould not be large; for though an increaſe of magnitude leſſens the trouble of cleaning, it at the ſame time increaſes the expence of repairing damages which frequently occur.

5th, The ſeveral wires ſhould be fixed very ſteadily, or in ſuch a manner as not to admit of any ſhaking.

6th, It ſhould take up the leaſt poſſible room; for as it grows in ſize, ſo is the probability increaſed of its being expoſed to the influence of ſurrounding conductors.

In the conſtruction of that battery which I uſe, I have given all the attention of which I am capable, to the preceding requiſites.

FIG. LII.

AB is an iron or braſs rod, whoſe length is regulated by the length of the table on which the battery is fixed. The wire ſhould not be leſs than one-third of an inch thick. The balls at its extremities may be made either of well poliſhed wood or metal, and they ſhould in diameter equal three inches.

FIG. LIII.

AB is a braſs or iron wire, one-fourth of an inch in diameter; at C, it is ſcrewed into the braſs ring, A; at B, hangs a piece of chain, which reſts on the bottom of the phial. The whole of this wire is connected with the rod AB, (Fig. LII.) The braſs ring may be moved, together with its appended wire, to any required poſition on the rod, and if it be neceſſary to take away the wire, it is eaſily unſcrewed. No amplification is neceſſary to ſhew that the contrivance I have juſt deſcribed may be repeated, and applied to as many jars as the length of the rod will allow to ſtand under it.

FIG. LIV.

The rod AB, in Fig. LII. is ſupported by reſting each extremity on a glaſs inſulator, ſuch as HI, which is faſtened to the table by iron ſcrews. The upper end of the inſulator is covered with a wooden cap, hollowed ſo as to receive the balls of the rod. As the number of jars may be increaſed by increaſing the length of the rod, ſo the battery may be magnified into any ſize by multiplying the rods.

The preceding is the moſt expenſive part of an electrical apparatus. Sometimes eight or ten phials are broken by one diſcharge, and they are not replaced without much time and trouble. Mr. Brooke has tried the various means preſcribed for curing the fractures of broken jars; but in nine inſtances out of ten, before the cured jars were twice charged, they broke a ſecond time at the edge of the cement that was laid on the glaſs. The preceding evil may be much diminiſhed by increaſing the ſize of the battery, ſo as to make a low charge adequate to the effect we wiſh to inveſtigate.

I have already given you a general deſcription of the diſcharging-rod which I uſe.* I ſhould obſerve that the greateſt difficulty in conſtructing it is that of making the axis turn perfectly tight in the collar; for the leaſt ſhaking, where the motion is ſo conſiderable, is ſoon followed by injury. As the inſtrument is of permanent and conſtant uſe, you will ſave trouble and expence by having recourſe to the ſkill of a mathematical inſtrument-maker. Mr. Blunt, of Cornhill, London, has conſtructed what you now ſee, and can ſupply you without any new directions. I might recommend one alteration: the wheels would be improved, by prefering a diameter of ſix, to that of three inches.

Mr. Brooke's electrometer is an expenſive inſtrument, and can be made only by a very ſkilful workman; it is, however, the only one of its kind that can be relied on. He has given a long and accurate deſcription of it in his treatiſe, entitled, "Miſcellaneous Experiments and Remarks on Electricity." It is from this deſcription alone that you can poſſibly conſtruct an inſtrument for yourſelves, as in that given in the Philoſophical Tranſactions, the whole is rendered unintelligible by a negligent omiſſion of the reference-letters in the plates.

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Having now made you acquainted with all parts of the apparatus that is neceſſary for exciting, collecting, and communicating the electric fluid, I ſhall proceed to the deſcription of thoſe teſts by which the nature of the accumulated electricity is determined.

When the electric is charged to a conſiderable height, it ſhould be placed on an inſulator, and the finger applied to one of its ſurfaces. A pair of pith balls, connected with a ſurface known to be charged poſitively or negatively, ſhould then be applied to it. If the poſitive or negative balls be repelled, then the electricity is poſitive; if attracted, the electricity is contrary to that of the balls. When the fluid is collected in very ſmall quantities, Mr. Bennet's electroſcope is, in my opinion, the beſt poſſible. The deſcription, however, which it was neceſſary for me to give of it at the commencement of theſe Lectures, differs in ſome reſpects from that given by the author.

1ſt, The ſhape of the gold ſtrip, if made according to his directions, muſt be triangular, the baſe of which is faſtened while the apex hangs down. I have always preferred a rectangular ſtrip, and I have choſen that the end which hangs down ſhould be rather broader than that which is faſtened, for it is beſt, I think, that the ſurface ſhould be greateſt where the influence is moſt powerful, or at the part which is moſt diſtant from the fulcrum.

2dly, Where Mr. Bennet places a cap of metal, I place a cake of ſealing-wax, through whoſe centre a tube paſſes, containing a wire, to one of whoſe ends is faſtened the ſtrip, to the other a piece of braſs, to which may be fitted a ball, a plate, or any kind of ſupport that the circumſtances of the experiment may require. By this conſtruction the inſulation is much improved, and conſequently the neceſſity diminiſhed of wiping the ſides of the glaſs, where the ſlighteſt friction is attended with ſigns of electricity. According to Mr. B.'s directions, the rim of the metallic cap hangs over the ſides of the glaſs, ſo that the falling duſt may not come near them. But this is no preſervative; for the electric accumulation in the inſide, be it ever ſo ſmall, cauſes an attractive influence, which gathers the floating particles in all directions.

Mr. Bennet's electroſcope is not eaſily moved, for the gold leaf ſtrips are injured by the leaſt ſhaking of the glaſs. If, therefore, you ſhould want an inſtrument of this kind for the pocket, I would recommend that invented by Mr. Cavallo, as the moſt convenient. Its ſenſibility may be much improved, if the pith balls, inſtead of being faſtened to ſilver wire, are ſuſpended by Mr. Read's flaxen threads.

FIG. LV.

ABCD is a ſmall phial, about two inches long, and about three-fourths of an inch wide. E is a braſs cap, which covers the neck of the phial. CD is a piece of metal cemented to the bottom, which may form part of the caſe convenient for the preſervation of the electroſcope, while in the pocket. I have found it altogether unneceſſary to put any tin foil on the ſide of the glaſs, both in this and Mr. B.'s electroſcope. The two flaxen threads, to which the balls are faſtened, are prepared in the following manner:

The flax is taken as it comes from the flax-comb, and ſplit ſo fine as almoſt to float in air. In this ſtate it is ſuſpended, having two weights appended to the lower end. The whole thread, thus ſtraitened, is drawn between the fingers, faſtened with glue, and when the glue is dry, the weights are ſtripped off. After this preparation, two pieces of the thread may be faſtened to the cork at D, and then moved into the bottle without danger.

The contrivances employed for making obſervations on natural electricity are as various as the different operators who have attended to the ſubject.

The moſt ſimple is a common fiſhing-rod, extended through the window of a houſe into the atmoſphere, having a great number of metallic briſtles faſtened to one end, which are connected with the other by a metallic wire paſſing through the cavity. This rod may be eaſily inſulated by fixing that part of it which is in the houſe to a glaſs ſtand. So much electric fluid is collected by this apparatus, when the leaſt change takes place in the air, that the gold leaf ſtrips, at a conſiderable diſtance from it, are moſt commonly torn to pieces by the violence of their ſeparation. To the inſulated end of the fiſhing-rod ſhould be annexed a metallic ball, and near it ſhould ſtand at all times a Leyden phial, together with every part of the apparatus which is neceſſary to determine the nature of the collected electricity; nor would it be amiſs, at the place of obſervation, to have a metallic wire connected with the well or water-pipe that ſupplies the houſe.

In caſe the conductor is to be perpetually active, an erect poſture is the beſt, as it admits of the beſt faſtenings and may be placed where its continuance may not be attended with any inconvenience.

FIG. LVI.

AB is a glaſs ſtem, not leſs than fourteen inches in length. It is to be fixed in a wooden rod, whoſe length is to be regulated by your own convenience. This rod is to reſt on a ſtrong inſulator, EF, which is fixed within the houſe. ACD is a tinned funnel, formed ſo as to ſhelter the inſulator, AB. Its apex ſhould terminate in a great number of diverging points. DG is a metallic communication, by which the fluid is conveyed from the funnel to H, where it is examined by the operator. Mr. Brooke, by a ſeries of obſervations made with conductors of different lengths, proved that in caſe ACD be well inſulated, its height is of no conſequence: I ſhould therefore recommend its being placed where the inſulator may be reached and wiped without difficulty. In caſe the wooden rod, BE, ſhould paſs through an orifice in the roof of your houſe, EF may be ſcrewed to your garret floor, and the wire, DG, conveyed through an opening in the cieling to the room below: but in caſe this is not convenient, the wire DG may be continued thro' the open air, and introduced through the window into any lower apartment.

Mr. Read and Mr. Bennet, when the ſigns of electricity are feeble, recommend the uſe of a torch applied to the end of the conductor. The torch is of itſelf adequate to the production of electricity, and by introducing what is collected artificially, as a natural effect, it may lead us into very great errors.

I meant at one time to have given you a ſeries of experiments, which might ſerve to ſhew the connexion between electricity and chemiſtry; but I ſhall content myſelf with deſcribing the apparatus to you, the deſign of its ſeveral parts, and the mode of uſing it. The numerous facts which have occurred to my obſervation, belong more properly to another ſubject.

CHEMICO ELECTRICAL APPARATUS.

When the electrical fluid is employed as the cauſe of decompoſitions and new combinations, great accumulations of it are neceſſary, and in proportion to the magnitude of the ſcale by which the powers we employ are to be meaſured, ſo will be the diſtinctneſs and deciſion of our reſults. No perſon, therefore, ſhould attempt to inveſtigate the connexion of chemiſtry and electricity without a large battery well conſtructed, together with a proper diſcharging-rod, and all the means which I have deſcribed as neceſſary to form a complete circuit. His apparatus, moreover, muſt have varieties ſuitable to the character of the bodies upon which his experiments are to be made. Fluids require an apparatus different from that which is applicable to ſolids. Some fluids, likewiſe, and ſome ſolids, are to be inveſtigated differently from others.

When the properties of fluids are examined, diſtinct modes of operation are required :

1ſt, By the various kinds of air.
2d, By water, ſpirits of wine, and æther.
3d, By the oils.
4th, By the acids.
5th, By mercury.

It ſhould be here obſerved, that as the power of heat, when applied in chemiſtry, requires the action of three ſeveral bodies, ſo in applying the electric fluid, we are always obliged to conſider the change it produces, not only in the body upon which it is deſigned to act, but in the body that conveys it, and in the medium ſurrounding that part of the circuit in which the exploſion is moſt powerful. Thus if I convey the charge of a battery through ſulphur, the conducting metal is affected, the ſulphur is burned, and the air which ſurrounds the ſulphur is at the ſame time rendered impure.

I. APPARATUS FOR VARIOUS KINDS OF AIR.

FIG. LVII.

AB is a tube about ſix inches long, half an inch wide, and as nearly cylindrical as poſſible. At the upper extremity is fitted to the tube the iron cap, CD, having two ſtop-cocks ſcrewed into its ſides, which communicate with the bladders, E and H. A thermometrical tube paſſes through a lid ſcrewed upon the iron cap, CD, into the cavity of the tube, AB. I and H are the conducting wires, which ſhould always be made of gold, as the ſubſtance that is leaſt affected by the force of the battery. B is a piſton, which I make of cotton twiſted round an iron wire; it would be better made of cork, ſo perfectly turned as to fit the bore of the tube, AB, which ſhould be completely cylindrical for the purpoſe. The mode of proceeding with this inſtrument is the following:

The lid of the iron cap is taken off, and the piſton is puſhed beyond the orifices of the ſtop-cock; the lid is then ſcrewed on, ſo as to be air-tight, and the communication with that bladder which contains the required air is opened. In conſequence of pulling down the piſton, the air ruſhes from the bladder, and fills the vacuum that is produced by the piſton, whoſe motion will be regulated by the ſtriking diſtance. The fluid is conducted by a ſmall wire, which paſſes through the thermometrical bore. The cement that makes it air-tight before the piſton is moved, may be torn off afterwards, and the wire may be moved backwards and forwards without danger of loſing much air. After the ſhock or ſpark has been repeated a certain number of times, the piſton, being puſhed up, will force the air in its changed ſtate into one of the bladders, which is left empty and open for the purpoſe of receiving it; a freſh ſupply is then introduced from the other bladder, and the operation goes on till the whole has undergone the ſame operation, or till the bladder, containing the required air, becomes empty. In caſe you wiſhed to renew and continue the application of the electric fluid, it is obvious to you that the bladder may be alternately filled and exhauſted, as often as you pleaſe.

II. APPARATUS FOR WATER, SPIRITS OF WINE, AND OIL.

FIG. LVIII.

If the ſhock be conveyed from one point to another, in water contained in glaſs, a fracture takes place, though the ſides of the glaſs be three inches thick. The apparatus repreſented in the figure is made with a deſign to avoid this inconvenience. A is a glaſs tube three inches long and half an inch in diameter. It is cemented into a cylindrical piece of wood, BC, which is perforated from top to bottom, and the diameter of its perforation is the diameter of the tube. Through the ſide of the wood a thermometrical tube, containing a wire (as in the preceding figure) paſſes to H. Through the bottom of the wood another wire paſſes, and extends to within one-fourth of an inch of H, or to I. The ſhock produces its greateſt effect in the interval between the two points, and whatever bubbles are formed, riſe through the water contained in the cavity to the ſummit of the tube, A. If the thermometrical tube be not placed at H, you will not have inſulation enough to prevent the ſhock from ſtriking from one wire to another along the wood, rather than through the water or ſpirits of wine.

III. APPARATUS FOR THE ACIDS.

FIG. LIX.

AB is a tube eight inches long and half an inch wide. It is cloſed at the top by a cap, through which paſſes a thermometrical tube, twiſted round with a gold wire, which terminates at G. The remainder of the thermometrical tube is covered with acid. E is the point of another piece of gold wire, which paſſes through the water in which the tube is inverted: in this caſe the fluid paſſes from G through the acid on the ſide of the thermometrical tube, and the greateſt ſenſible change is produced on the air in the tube AB. I have frequently uſed a bent tube, which I have filled with the acid, leaving only a bubble of air, through which the ſtroke was conveyed; but the inconſiderable conducting power of the acid in theſe circumſtances would never allow of my applying ſuch a force as produced any ſenſible change without immenſe trouble. The reſults of the experiments made on acids by this inſtrument are much varied by introducing different kinds of air into the tube.

IV. APPARATUS FOR SOLIDS.

When the ſolid is capable of being reduced into powder, it ſhould be preſſed cloſely into a tube, and one of the conducting points is to lie juſt above the ſurface, ſo that the air may have an opportunity of co-operating with the electric fluid. The conducting points ſhould be always placed within the luminous ſtriking diſtance. The tube ſhould be cloſed; for the air is changed as well as the body, and different reſults take place in different kinds of air. Salts may be examined, if they be firſt diſſolved in water, and the ſhock through the water thus impregnated. Metals diſplay an endleſs variety of reſult, when acted upon by the electric fluid: fine wires of them are to be faſtened to little hooks at H and I (vide Fig. LVII.) in the tube AB, which may be filled with any fluid, ſo that the effects of the ſtroke upon the metal in that fluid may be examined.

When the wires are melted or decompoſed in different kinds of air, my method of proceeding is the following:——I puſh up the piſton, ſo that the little hook at I may lie beyond the orifice of the ſtop-cock; I then tye the wire of the required metal to the hook, and ſcrew on the iron lid, to whoſe thermometrical tube likewiſe the wire is faſtened. On pulling down the piſton, the wire is drawn into a tight poſture, and the required air enters the cavity. After the diſcharge, the air changed by the experiment may be puſhed into one of the two bladders, and the metal, or its remains, will be found on the ſurface of the piſton. It is obvious that this operation may be repeated times without number in the ſame air, ſo that the maximum of this effect may be thus eaſily and accurately aſcertained.

By ſubſtituting oil and other fluids for air, the preceding proceſſes on the metals may be diverſified with great facility. Indeed, the experiments which may be made with the whole of the apparatus I have juſt deſcribed, if properly varied, might furniſh employment for a long life of activity and perſeverance.

FINIS.

PLATE 1

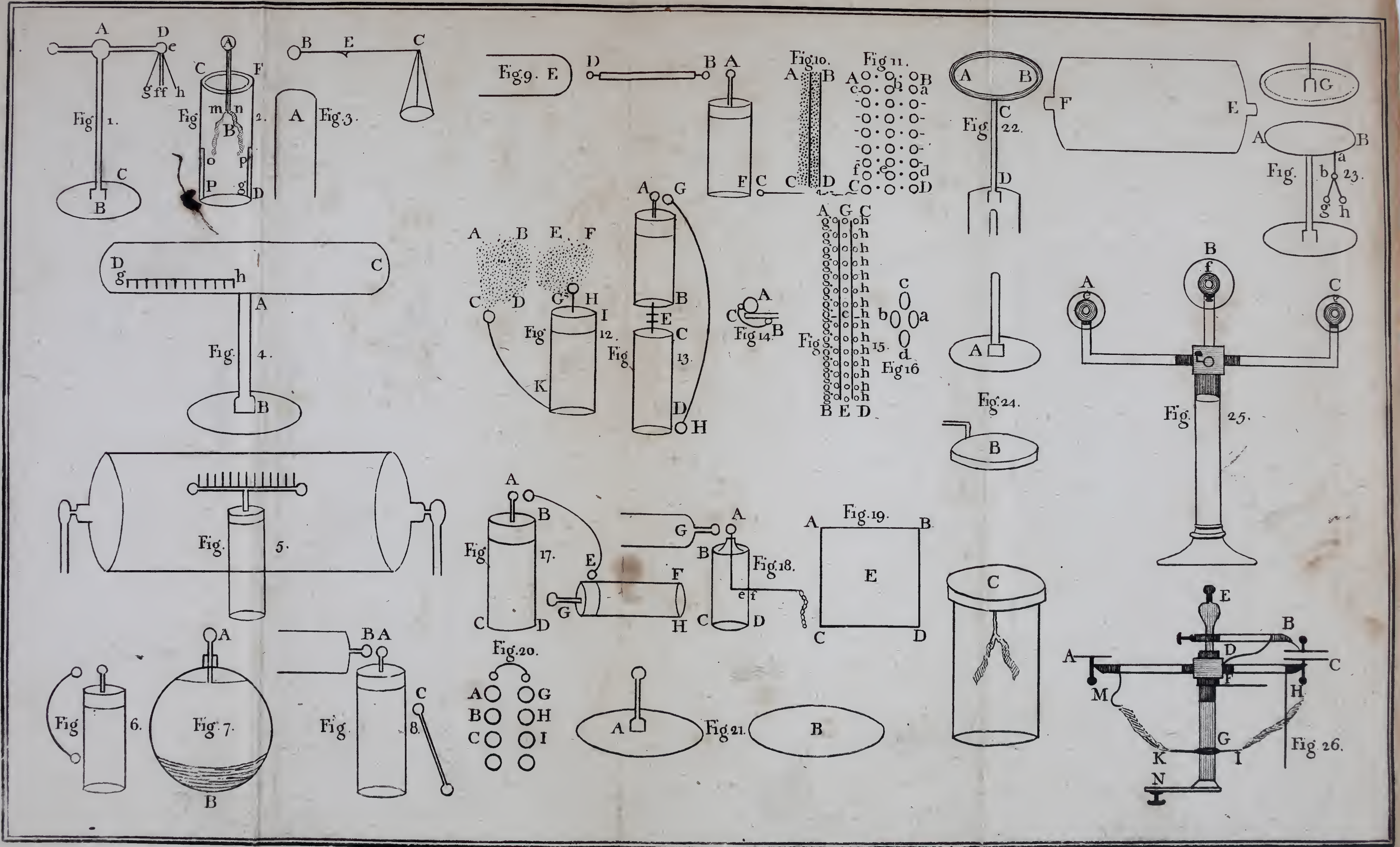
Fig. 1. A D e g ff h C B
Fig. 2. A C F m n B o p P g D
Fig. 3. A B E C
Fig. 4. D g h C A B
Fig. 5.
Fig. 6.
Fig. 7. A B
Fig. 8. B A C
Fig. 9. E
D B A F C
Fig. 10. A B C D
Fig. 11. A c b B a f e d C D
Fig. 12. A B E F C D G H I K
Fig. 13. A G B E C D H
Fig. 14. A C B
Fig. 15. A G C g o h e B E D
Fig. 16. c b a d
Fig. 17. A B E F G C D H
Fig. 18. G A B e f C D
Fig. 19. A B E C D
Fig. 20. A B C G H I
Fig. 21. A B
Fig. 22. A B C D
F E
Fig. 23. G A B a b g h
Fig. 24. A B
C
Fig. 25. A B C e f g
Fig. 26. E B D A C M F H G K I N

Plate 2

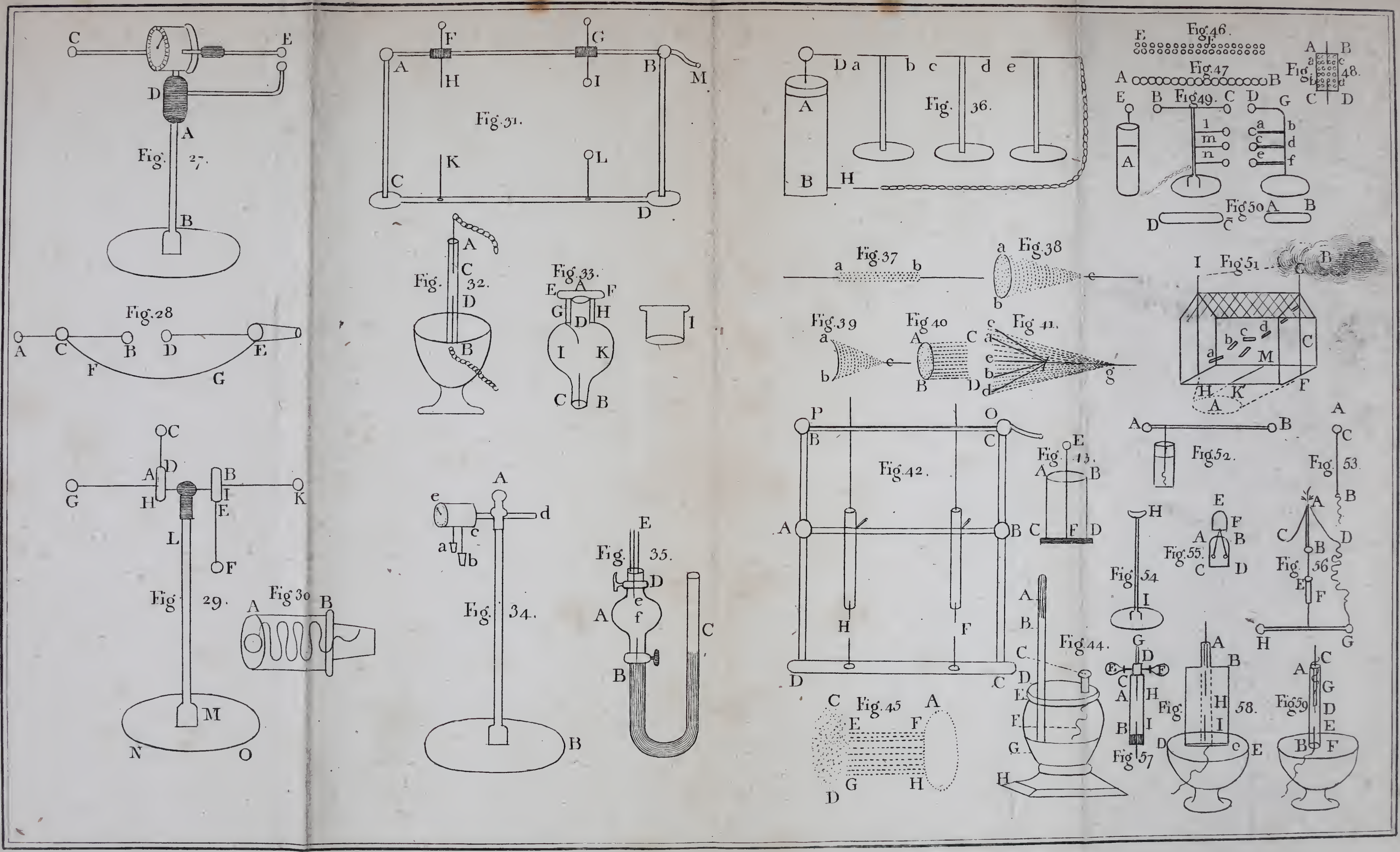

Fig. 27.
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Fig. 28
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Fig. 29.
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Fig 30
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Fig. 31.
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Fig. 32.
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Fig. 33.
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Fig. 34.
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Fig. 35.
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Fig. 36.
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Fig. 37
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Fig. 38
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Fig. 39
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Fig 40
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Fig 41.
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Fig. 42.
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Fig. 43.
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Fig. 44.
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Fig. 45
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Fig. 46.
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Fig. 47
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Fig. 48.
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Fig. 49.
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Fig. 50
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Fig. 51
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Fig. 52.
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Fig. 53.
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Fig. 54
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Fig. 56
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Fig. 57
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Fig. 58.
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Fig. 59
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